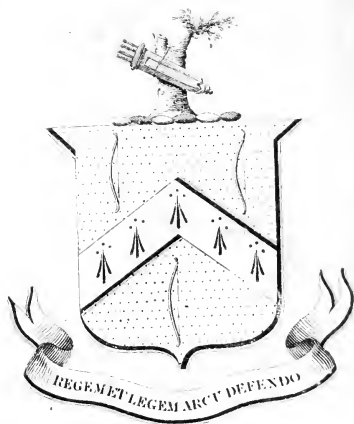


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R. B. Bowman.



At a Meeting of the Council of the Royal Society,
Feb. 28, 173²₃.

*THE greatest Part of this Second
Volume of Statical ESSAYS, by
Stephen Hales, B. D. F. R. S. having
been read at several Meetings of the
Royal Society, for which he had their
Thanks, it is ordered that he be de-
sired to Print the same.*

Hans Sloane, P. R. S.

Physical

Statical ESSAYS:

CONTAINING

HÆMASTATICKS;

OR,

An Account of some HYDRAULICK and
HYDROSTATICAL Experiments made on
the Blood and Blood-Vessels of Animals.

ALSO

An Account of some Experiments on Stones in the
Kidneys and Bladder; with an Enquiry into
the Nature of those anomalous Concretions.

To which is added,

An *A P P E N D I X*,

CONTAINING

OBSERVATIONS and EXPERIMENTS relating
to several Subjects in the first Volume. The
greatest Part of which were read at several
Meetings before the Royal Society.

With an INDEX to both VOLUMES.

V O L. II.

*Desideratur Philosophia Naturalis vera & activa cui
Medicinæ Scientia inedicetur.*

Fran. de Verul. Instaur. Magna.

By STEPHEN HALES, B D. F. R. S.
Rector of *Farringdon*, *Hampshire*, and
Minister of *Teddington*, *Middlesex*.

L O N D O N:

Printed for W. INNYS and R. MANBY, at the West-
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Rec'd Mch 1907

TO THE
K I N G ' s

Most Excellent Majesty.

S I R,

YOUR Majesty's gracious Acceptance of my former Volume of Experiments, has encouraged me, both further to pursue these natural Researches, and also

A 3 to

to lay the Result of them at Your Feet.

The Study of Nature will ever yield us fresh Matter of Entertainment, and we have great reason to bless God, for the Faculties and Abilities he has given us, and the strong Desire he has implanted in our Minds, to search into and contemplate his Works, in which the farther we go, the more we see the Signatures of his Wisdom and Power, every Thing pleases and instructs us, because in every Thing we see a wise Design.

As

As the beautiful Fabrick of this World was chiefly framed for and adapted to the Use of Man, so the greater insight we get into the Nature and Properties of Things, so much the more Beneficial will they be to us, the more will our real Riches thereby increase, the more also will Man's original Grant of Dominion over the Creatures be enlarged.

Your Majesty's Subjects of *Great Britain* are allowed by the candid Confession of other Nations, to excell in experimental Philosophy, which has

long been found to be most beneficial to Mankind.

As the Advancement of Arts and Sciences much depends on the Protection of Princes, whose Patronage they are well worthy of; so we have a pleasing Prospect of their flourishing under Your Majesty's auspicious Favour, whose Care and Concern for the Welfare and Prosperity of His People, is in every respect most extensive.

That Your Majesty, after having long continued a
Blessing

Blessing to Your Subjects in
a prosperous Reign here on
Earth, may hereafter enjoy
a happy Immortality in Hea-
ven, is the sincere Prayer
of,

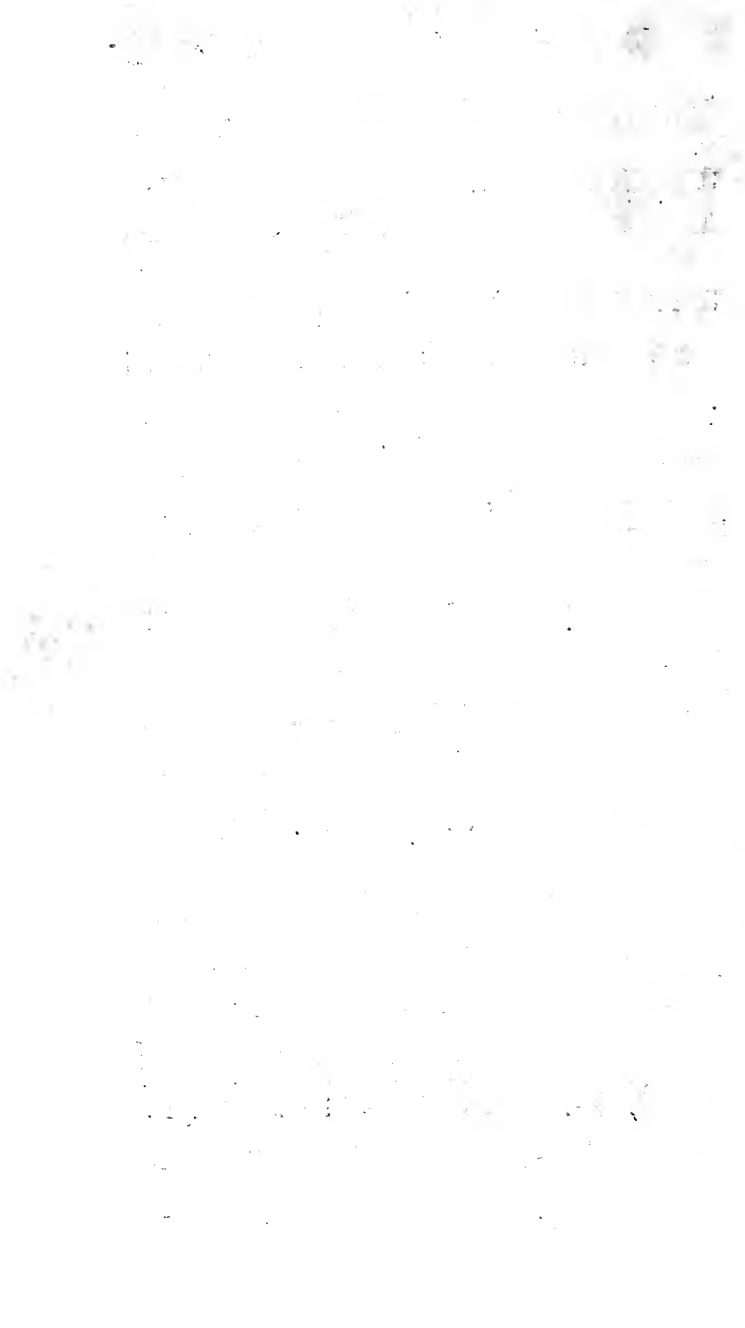
May it please Your Majesty,

Your Majesty's

most humble and

dutiful Subject,

STEPHEN HALES.



T H E
P R E F A C E.

W H A T I had at first intended only as additional Observations and Experiments to the first Volume, is now grown into the Size of another Volume, so fruitful are the Works of the great Author of Nature in rewarding by farther Discoveries, the Researches of those *who have Pleasure therein*: We can never indeed want Matter for new Experiments; and tho' the History of Nature as recorded from almost innumerable Experiments, which have been made within the compass of a Century, be very large, yet the Properties of Bodies are so various, and the different Ways by which they may be
examined

examined so infinite, that 'tis no wonder that we are as yet got little farther than to the Surface of Things: Yet ought we not to be discouraged, for tho' we can never hope to attain to the compleat Knowledge of the Texture, or constituent Frame and Nature of Bodies, yet may we reasonably expect by this Method of Experiments, to make farther and farther Advances abundantly sufficient to reward our Pains.

And tho' this Method be tedious, yet our Abilities can proceed no faster; for as the learned Author of the *Procedure of human Understanding* observes, *pag.* 205, 206.
 “ All the real true Knowledge we
 “ have of Nature is intirely *experi-*
 “ *mental*, infomuch that, how
 “ strange soever the Assertion seems,
 “ we may lay this down as the first
 “ fundamental

“ fundamental unerring Rule in
“ *Phyicks, That it is not within*
“ *the compass of human Understand-*
“ *ing to assign a purely speculative*
“ *Reason for any one Phænomenon*
“ *in Nature.*” So that in natural
Philosophy, we cannot depend on
any meer Speculations of the Mind;
we can only with the Mathemati-
cians, reason with any tolerable Cer-
tainty from proper *Data*, such as
arise from the united Testimony
of many good and credible Expe-
riments.

Yet it seems not unreasonable on
the other hand, tho’ not far to in-
dulge, yet to carry our Reasonings
a little farther than the plain Evi-
dence of Experiments will warrant;
for since at the utmost Boundaries
of those Things which we clearly
know, there is a kind of Twilight
cast

cast from what we know, on the adjoining Borders of *Terra incognita*, it seems therefore reasonable in some degree to indulge Conjecture there; otherwise we should make but very slow Advances in future Discoveries, either by Experiments or Reasoning: For new Experiments and Discoveries do usually owe their first Rise only to lucky Guesses and probable Conjectures, and even Disappointments in these Conjectures, do often lead to the Thing sought for: Thus by observing the Errors and Defects of a first Experiment in any Researches, we are sometimes carried to such fundamental Experiments, as lead to a large Series of many other useful Experiments and important Discoveries.

If therefore some may be apt to think that I have sometimes too far

far indulged Conjecture, in the Inferences I have drawn from the Events of some Experiments; they ought to consider that it is from these kind of Conjectures that fresh Discoveries first take their Rise; for tho' some of them may prove false, yet they often lead to further and new Discoveries. It is by the like Conjectures that I have been led on, Step by Step, thro' this long and laborious Series of Experiments; in any of which I did not certainly know what the Event would be, till I had made the Trial, which Trial often led on to more Conjectures and farther Experiments.

In which Method we may be continually making farther and farther Advances in the Knowledge of Nature, in proportion to the Number of Observations which we have:

But

BUT as we can never hope to be furnished with a sufficient Number of these, to let us into a thorough knowledge of the great and intricate Scheme of Nature, so it would be but dry Work to be ever laying Foundations, but never attempting to build on them. We must be content in this our infant State of Knowledge, while we *know in part* only, to imitate Children, who for want of better Skill and Abilities, and of more proper Materials, amuse themselves with flight Buildings. The farther Advances we make in the Knowledge of Nature, the more probable and the nearer to Truth will our Conjectures approach : so that succeeding Generations, who shall have the Benefit and Advantage both of their own Observations, and those of preceding Generations, may then

then make considerable Advances, when *many shall run to and fro, and Knowledge shall be increased,* Dan. xii. 4. In the mean time, it would but ill become us in this our State of Uncertainty, to treat the Errors and Mistakes of others with Scorn and Contempt, when we cannot but be conscious, that we ourselves see Things *but as thro' a Glass darkly,* and are very far from any Pretensions to Infallibility.

As it has ever been of great Importance to the Welfare of Mankind, to make the best Researches they can into the Nature of our Bodies, so have many eminent Persons, from time to time, made considerable Discoveries therein. And as the animal Fluids move by Hydraulick and Hydrostatical Laws, so I have here made some enquiry into the Nature of their

Motions by a fuitable Series of Experiments. The disagreeableness of the Work did long discourage me from engaging in it; but I was on the other hand spurred on by the hopes that we might thereby get some farther Insight into the animal OEconomy. We find here a large Field for Experiments, which may be multiplied and varied many Ways, of which I have here only given a few Specimens. As these Experiments do obviously and clearly give an Account of some *Phænomena*, so they may possibly be of Service in the Hands of those who are well skilled in the animal OEconomy and the History of Diseases, to explain many other of the innumerable Variety of Cases, which occur in so complicated a Subject as an animal Body is.

In

In which we are assured that all Things are wisely adjusted in Number, Weight and Measure, yet with such complex Circumstances as require many *Data* from Experiments, whereon to found just Calculations: But tho' many of the following Calculations are founded only on such inaccurate Mensurations as the Nature of the Subject would allow of, yet may we thence fairly draw many rational Deductions in relation to the animal OEconomy.

In which there is so just a Symmetry of Parts, such innumerable Beauties and Harmony in the uniform Frame and Texture of so vast a Variety of solid and fluid Parts, as must ever afford Room for farther Discoveries to the diligent Enquirer; and thereby yield fresh Instances to illustrate the Wisdom of the divine

Architect, the Traces of which are so plain to be seen in every Thing, that the *Psalmist* had good reason to call him a *Fool* who could be so senseless as to *say in his Heart that there is no God*; whose masterly Hand is so evident in every Part of Nature, that if there be any who pretend they cannot see it, it can be no Breach of Charity to say that they are wilfully blind, and therefore Lyars.

In the Treatise on the *Calculus*, I have endeavoured by a great Variety of Experiments, to enquire into the Nature of that most formidable Concretion; but tho' I have not thence been able, to find out either a sure Preventive, or a safe Dissolvent, yet I am not without hopes that these Researches, as they were sincerely intended, so they will prove of considerable Benefit to Mankind in many Cases,

Cases, by shewing not only the Nature, but also the Causes which are most apt, either to promote or retard the Progress of this Secret and terrible Intruder; not that it is to be expected, that all should find Benefit thereby, tho' haply some may. The Instrument described at the End of this Treatise, will, I doubt not, be very beneficial in instantly relieving many, who might otherwise suffer great Torture for several Days, and some of them lose their Lives by a dangerous Incision.

The Appendix contains several miscellaneous Observations and Experiments, some of which relate to Subjects in *Vegetable Statics*, but the greatest Part to the *Analysis* of the Air in the first Volume.

I have added a general Index of the Matters contained in these two

Volumes. But as this Index is adapted to the second Edition of the first Volume; so there must be an Allowance for a difference of eight Pages less, from Page 75 of the first Edition of the first Volume; which difference in the numbering of the Pages of the two Editions of the first Volume, continually decreases, so as to be none in the last Page of each Edition.

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THE

T H E INTRODUCTION.

1. **A**S an animal Body consists not only of a wonderful texture of solid Parts, but also of a large proportion of Fluids, which are continually circulating and flowing, thro' an inimitable Embroidery of Blood-Vessels, and other inconceivably minute Canals : And as the healthy State of an Animal principally consists, in the maintaining of a due *Equilibrium* between those Solids and Fluids; it has, ever since the important Discovery of the Circulation of the Blood, been looked upon as a Matter well worth the enquiring into, to find the Force and Velocity with which these Fluids are impelled; as a likely means to give a considerable Insight into the animal OEconomy.

2. Several ingenious Persons have from time to time, attempted to make Estimates of the Force of the Blood in the Heart and Arteries, who have as widely differed from each other as they have from the Truth, for want of a sufficient Number of *Data* to argue from : Had Persons of their Abilities been
been

The INTRODUCTION.

been more careful, in the first Place to get what Insight they could into the Matter, as far as a regular Series of proper Experiments would have informed them, they would then doubtless have been furnished, with more and more proper *Data* whereon to found their Calculations, which would have brought them much nearer to the Truth.

3. Finding therefore but little Satisfaction, in what had been attempted on this Subject by *Borellus* and others, I endeavoured about twenty-five Years since by proper Experiments, to find what was the real Force of the Blood in the crural Arteries of Dogs, and about six Years afterwards I repeated the like Experiments on two Horses, and a fallow Doe ; but did then not pursue the Matter any further, being discouraged by the disagreeableness of anatomical Dissections. But having of late Years found by Experience the Advantage of making use of the Statical Way of Investigation, not only in our Researches into the Nature of Vegetables, but also in the chymical Analysis of the Air; I was hence induced to hope for some Success, if the same Method of Enquiry were applied to animal Bodies;

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Bodies ; especially considering that they are in a manner framed of one continued Maze of innumerable Canals, in which Fluids are incessantly circulating, some with great Force and Rapidity, others with very different Degrees of rebated Velocity : Hence I was encouraged to resume the Enquiry, by Variety of such Experiments as I conjectured would give some Light into the Matter.

4. It may well be wondered at, that one who neither by Profession nor Inclination, is prompted to deal in anatomical Operations, should engage in a Thing of this Nature ; especially in an Age and Nation that abounds with ingenious Persons, who are eminently skilled in Anatomy ; and who have brought the Art of preparing and injecting the finer capillary Vessels, with proper coloured Ingredients, to a good Degree of Perfection.

5. But as the animal Canals have not, that I know of, been hitherto examined any other Way, than by the imperfect and uncertain Methods of injecting with a Syringe and blowing into them ! I hope it will appear from this Specimen to be worth the while to attempt it in the following

Hydraulick

The INTRODUCTION.

Hydraulick Way, which seems to me to be more accurate and certain. And I hope that what I have here done in this Way will induce more skilful Anatomists to pursue the Matter farther by a great Variety of the like Experiments on different Parts of the Body, and also with Variety of Liquors, of very different Degrees of Consistency, as to their Viscidity or Thinness and Fluidity, Restrictingency and other Qualities; whence I make no doubt but many curious Observations and Discoveries would arise. For since we are assured that the animal Fluids move by *Hydraulick* and *Hydrostatick* Laws, the likeliest Way therefore to succeed in our Enquiries into the Nature of their Motions, is by adapting our Experiments to those Laws.

6. So complicated and curiously wrought a Fabrick as an animal Body is, the admirable and amazing Texture of every Part of which, declares its divine Original, and whose Welfare and good Order consisting in the concurring Harmony of such innumerable Circumstances, will, in whatever View we consider it, ever afford fresh Matter to reward our Researches.

AN
ACCOUNT
OF SOME

Hydraulick and Hydrostatical EXPERIMENTS made on the Blood and Blood-Vessels of ANIMALS.

EXPERIMENT I.

1. **I**N *December* I caused a Mare to be tied down alive on her Back, she was fourteen Hands high, and about fourteen Years of Age, had a *Fistula* on her Withers, was neither very lean, nor yet lusty: Having laid open the left crural Artery about three Inches from her Belly, I inserted into it a brass Pipe whose Bore was one sixth of an Inch in Diameter; and to that, by means of another brass Pipe which was fitly adapted to it, I fixed a glass Tube, of nearly the same Diameter, which was nine Feet in Length: Then untying the Ligature on the Artery, the Blood rose in the

B

Tube

Tube eight Feet three Inches perpendicular above the Level of the left Ventricle of the Heart : But it did not attain to its full Height at once ; it rushed up about half way in an Instant, and afterwards gradually at each Pulse twelve, eight, six, four, two, and sometimes one Inch : When it was at its full Height, it would rise and fall at and after each Pulse two, three, or four Inches ; and sometimes it would fall twelve or fourteen Inches, and have there for a time the same Vibrations up and down at and after each Pulse, as it had, when it was at its full Height ; to which it would rise again, after forty or fifty Pulses.

2. The Pulse of a Horse that is well, and not terrified, nor in any Pain, is about thirty six Beats in a Minute, which is nearly half as fast as the Pulse of a Man in Health : This Mare's Pulse beat about fifty five times in a Minute, and sometimes sixty or a hundred she being in pain.

3. Then I took away the glass Tube and let the Blood from the Artery mount up in the open Air, when the greatest Height of its Jet was not above two Feet.

4. I measured the Blood as it run out of the Artery, and after each Quart of Blood was run out, I refixed the glass Tube to the
Artery

Artery, to see how much the Force of the Blood was abated ; this I repeated to the eighth Quart, and then its Force being much abated, I applied the glass Tube after each Pint had flowed out : The Result of each Trial was as is set down in the following Table, in which are noted the greatest Heights it reached after every Evacuation : It was usually about a Minute before it rose to these several Heights, and did not rise gradually, but would stand during several Pulses much lower, than what it would at length reach to ; so that I often thought it had done rising, when on a sudden it would rise for some time four, eight, twelve or sixteen Inches higher, where it would stay for some time, and then on a sudden fall four, eight, twelve or sixteen Inches.

• These 5 Ounces lost in preparing the Artery.

By this time there is a Pint lost in making the several Trials, which is not allowed for in this Table.

The several Trials.	The Quantities of Blood let out in Wine Measure.		The several Heights of the Blood after these evacuations	
	Quarts	Pints	Feet	Inches
1	0	* 5 Ounces	8	3
2	1	0	7	8
3	2		7	2
4	3		6	6 $\frac{1}{2}$
5	4		6	10 $\frac{1}{2}$
6	5		6	10 $\frac{1}{2}$
7	6		5	5 $\frac{1}{2}$
8	7		4	8
9	8		3	3
10	8	I	3	7 $\frac{1}{2}$
11	9	0	3	10
12	9	I	3	6 $\frac{1}{2}$
13	10	0	3	9 $\frac{1}{2}$
14	10	I	4	3 $\frac{1}{2}$
15	11	0	3	8
16	11	I	3	10 $\frac{1}{2}$
17	12	0	3	9
18	12	I	3	7 $\frac{1}{2}$
19	13	0	3	2
20	13	I	4	$\frac{1}{2}$
21	14	0	3	9
22	14	I	3	3
23	15	0	3	4 $\frac{1}{2}$
24	15	I	3	I
25	16	0	2	4

There was about a Quart lost in making the several Trials, so there flowed out in all seventeen Quarts, and half a Pint after the last Trial, when she expired. This whole Quantity of Blood was equal to 1185.3 cubick Inches.

5. We may observe from this Table, that the Decrease of the Force of the Blood in the Arteries, was not proportioned to the several Quantities of Blood which were evacuated; for at the eighth Trial, when seven Quarts were drawn off, the Height of the Blood was four Feet eight Inches, after which it decreased in the five following Trials to three Feet odd Inches, sometimes a little lower and then a few Inches higher. But at the fourteenth Trial, after ten Quarts and a Pint had been drawn off, it rose again up to four Feet $3 + \frac{1}{2}$ Inches, and it came nearly to the same Height again at the twentieth Trial when thirteen Quarts and a Pint had been drawn off.

6. This disproportionate Inequality in the several Heights was principally owing to her violent straining to get loose, which made the Blood in the fourteenth Trial rise higher than it had done in several of the preceding ones.

7. About the twentieth Trial she grew very faint and uneasy, and breathed quick: The violent straining to get loose, did by the acting of most of her Muscles, especially the abdominal, impell the Blood from all Parts to the *Vena Cava*, and consequently there

was a greater Supply for the Heart, which must therefore throw out more at each Pulsation, and thereby increase the Force of the Blood in the Arteries.

8. For the same Reason too, it would be somewhat increased in Height upon deep sighing, because the Lungs being then put into greater Motion and more dilated, the Blood passed more freely and in greater Quantity, to the left Auricle and thence to the Ventricle,

9. This plainly shews how sighing increases the Force of the Blood, and consequently proportionably cheers and relieves Nature, when oppressed by its too slow Motion, which is the Case of those who are dejected and sad,

10. Hence also we see evidently, that the Blood moves fastest and most freely thro' the Lungs when they are in a dilated State: For which Reason Animals when they are near expiring, do usually breathe quick, the Lungs then labouring to heave fast, that the languid Blood may thereby have a freer Course thro' them, to supply the then almost bloodless Pulsations of the Heart, as was we see the Case of this Mare when her Blood was near exhausted.

11. When between fourteen and fifteen Quarts of Blood had been evacuated, and thereby the Force of that which remained in the Vessels greatly decreased, then the Mare fell into cold clammy Sweats, such as frequently attend dying Persons; which shews to how low a State the vital Force of the Blood is at that Time reduced: Whence we see, that these faint Sweats are not occasioned by a greater protrusive Force of the Blood at that Time, but rather by a general Relaxation of the Pores, as well as of all other Parts of the Body. And it seems hence probable, that the Vigour of the Blood in the Arteries is much abated, when Persons who are not in a dying State, have colliquative Sweats, as in violent Colic Pains, Fear, &c.

12. Upon opening the Mare's Body, I found little or no Blood in the *Aorta*, about an Ounce in the left Ventricle, but none in the right; the *Vena Porta* and *Cava* were full: She bled two or three Ounces, but very slowly, and not without pressing the jugular Vein, which was opened as soon as she expired.

13. There might be about two Quarts and three Quarters of Blood left in the large

Veins, which, with what was drawn out at the Artery, makes up twenty Quarts, equal to 1154 cubick Inches, or forty four Pounds; which at a low Estimation, may be reckoned the Quantity of current Blood in a Horse; there is doubtless considerably more, but it is not easy to determine how much.

14. As this Experiment shews how much the Force of the Blood in the Arteries is abated by different Degrees of Evacuation; so it may be of use to direct what Quantity to let out at a Time in bleeding: For whatever the real Quantity of the circulating Blood be, it is certain that the Estimate of what can be with safety let out at once, must be taken from the Proportion which that bears to the whole Quantity of Blood, which will flow out of the Vein or Artery of the Animal till it dies.

15. We see also from this Experiment, the Reasonableness of the Practice of bleeding at several distant Times, where it is requisite to take away a great Quantity of Blood, and not to do it all at once, which would too much weaken the Force of the Blood. For since it was found by several Instances in this Experiment, that when the Force of the
Blood

Blood was much depressed by Evacuations, it would be considerably raised again by the Action of the Muscles, out of whose very fine and long capillary Vessels it moves but slowly, as also by the Motion of all Parts of the Mare; so the Case is doubtless the same, when the Vigour of the Blood is in any Degree rebated in the large Vessels, by Blood-letting, that Vigour will in some measure be in a little Time restored again, not only by the Action of the several Parts of his Body, whereby the Blood would have time to flow in from all Parts, to supply the most evacuated Vessels, whereby there would be a just proportionate Evacuation of all Parts; but also because the Vessels themselves would thereby have time to contract themselves in some Proportion to the Degree of their Evacuation.

EXPERIMENT II.

- I. **I**N *January* I caused a Gelding to be tied down fast on his Back, in the same manner as the Mare was in the foregoing Experiment: He was thirteen Hands high, and ten or eleven Years old, but very lame, by reason of a Canker

a Canker in his Hoof; he was lean, but somewhat lustier than the Mare, and much more lively. I fixed the same Brass Pipe and Glass Tube, as above, to his left crural Artery.

2. The Blood rushed up the Tube at once, to near two thirds of its greatest Height, and then more leisurely as in the Mare. It would rise and fall commonly about an Inch at each Pulsation of the Heart, but sometimes two or three Inches. I let out the Blood gradually as in the Mare, and after each Evacuation, I refixed the Glass Tube to the Artery, to take the several Heights of the Blood: The Result of each Trial was as noted in the following Table.

3. When I first fixed the Tube to the Artery, I stopped the Horse's Nostrils, so as to make him breathe with great Difficulty, which made the Blood rise five Inches higher; but I could not carry this Experiment almost to Suffocation, as I would have done, because his plunging obliged me to take the Tube from the Artery.

The several Trials.	The Quantity of Blood let out.		The several Heights of the Blood after those Evacu-ations.	
	Quarts.	Pints.	Feet.	Inches.
1	0	1	9	8
2	1		9	8
3	2		9	5½
4	3		8	4
5	4		8	2
6	5		7	8½
7	6		7	1
8	7		7	6½
9	8		7	4½
10	9		6	6½
11	10		6	7¾
12	11		5	11
13	12		* 5	8½
	12		† 4	5½
14	13		4	4
15	14		3	8
16	14	1	* 4	2
	14	1	† 3	2
17	15		3	3½
18	15	1	2	10

* The highest Point it would stand at for some Time.

† The lowest Point; at which Points it would continue for some time.

* The highest Point.

† The lowest Point.

He did not bleed half a Pint more after this last Trial, before he expired.

4. We may observe, that as this Horse was more lively than the Mare, so the Blood mounted at first Trial seventeen Inches higher in the Tube, than the Mare's Blood did; yet there flowed three Pints of Blood less from

the Horse than from the Mare : One Reason of this may be, that as she was four Inches taller than the Horse, so she was probably proportionably bigger in Size every way, and should therefore have more Blood ; besides, Bulk for Bulk, the Females are observed to have more Blood than the Males.

5. As the Quantity of Blood decreased, so would the projectile Force of the Blood in the Tube proportionably decrease ; so that it would not rise above a quarter of an Inch at a Pulse, when the Horse grew very faint.

6. The great Ascents or Descents of the Blood, *viz.* twelve or fifteen Inches at a Time, did not seem to be owing immediately to the more vigorous or faint, faster or slower Pulsation or Sístole of the Heart ; but by its continued equable beating, seemed rather to be occasioned by a more or less Quantity of Blood flowing in to supply the left Ventricle of the Heart.

7 The Horse's Pulse beat forty Strokes in a Minute, before he was disturbed or tied down : But when the Glass Tube was fix'd to the Artery, it beat sixty five in a Minute. And as the Horse grew fainter, the Pulse was more and more accelerated, so as to beat an hundred times,
or

or more in a Minute: Whence we see, that the Pulse is weak and quick, when the Heart is supplied with little Blood; which is the Case in the Hæctick Fevers, &c.

8. And the Diastole of the Heart must necessarily be proportionably small; for if the Heart dilated as much, when a small Quantity of Blood flowed into the Ventricle, as when a large Quantity entered, it must then consequently be filled partly with Air each Time, which would soon cause the Death of the Animal.

EXPERIMENT III.

1. **I**N *December* I laid a common Field Gate on the Ground, with some Straw upon it, on which a white Mare was cast on her right Side, and in that Posture bound fast to the Gate; she was fourteen Hands and three Inches high; lean, tho' not to a great Degree, and about ten or twelve Years old. This and the above-mentioned Horse and Mare were to have been killed, as being unfit for Service.

2. Then laying open the left Jugular Vein, I fixed to that part of it which comes from
the

the Head, a Glass Tube, which was four Feet, and two Inches long.

3. The Blood rose in it, in three or four Seconds of Time, about a Foot, and then was stationary for two or three Seconds; then in three or four Seconds more, it rose sometimes gradually, and sometimes with an unequally accelerated Motion nine Inches more, on small Strainings of the Mare: Then upon greater Strainings it rose about a Yard, and would subside five or six Inches: Then upon a larger Strain or Struggle of the Mare, it rose so high, as to flow a little out at the Top of the Tube; so that had the Tube been a few Inches higher, it would have risen probably to that Height.

4. When the Mare ceased to strain and struggle, the Blood subsided about eighteen or twenty Inches; so the Return of the Blood into the Vein was not hindered by the Valves; which I have also observed in other Parts where there are Valves, tho' sometimes they absolutely hinder the Return of any Fluid.

5. The Diameter of the Brass Pipe and Tube which were fixed to the Vein, were nearly one seventh of an Inch: The Diameter of the Jugular Vein about half an Inch.

6. Then

6. Then laying bare the left Carotid Artery, I fixed to it towards the Heart the Brass Pipe, and to that the Wind-Pipe of a Goose; to the other End of which a Glass Tube was fixed, which was twelve Feet nine Inches long. The Design of using the Wind-Pipe was by its Pliancy to prevent the Inconveniencies that might happen when the Mare struggled; if the Tube had been immediately fixed to the Artery, without the Intervention of this pliant Pipe.

7. There had been lost before the Tube was fixed to the Artery, about seventy cubick Inches of Blood. The Blood rose in the Tube in the same manner as in the Case of the two former Horses, till it reached to nine Feet six Inches Height. I then took away the Tube from the Artery, and let out by Measure sixty cubick Inches of Blood, and then immediately replaced the Tube to see how high the Blood would rise in it after each Evacuation; this was repeated several times, till the Mare expired, as follows, *viz.*

	The several Trials.	Cubick Inches let out.	Perpendicular Height after each Evacuation.	
			Feet.	Inches.
	1	70	9	6
	2	130	7	10
	3	190	7	6
	4	250	7	3
	5	310	6	5
	6	370	4	9
	7	430	3	9
	8	490	3	4 $\frac{1}{2}$
	9	550	2	9 $\frac{1}{2}$
	10	610	3	2 $\frac{1}{2}$
* Deep Sighing raised the Blood.	11	670	* 4	5
			2	9 $\frac{1}{2}$
When the Force of the Blood was thus small, then faint Sweats came on. Very faint.	12	730	3	6
	13	790	3	5
	14	820	2	0
	15	833	2	5
Now expired.				

8. We may observe, that these three Horses all expired, when the perpendicular Height of the Blood in the Tube was about two Feet.

9. These 833 cubick Inches of Blood weigh 28.89 Pounds, and are equal to fourteen Wine Quarts, the large Veins in the Body of the

the Mare were full of Blood, there was some also in the descending *Aorta*, and in both Ventricles and Auricles.

10. In order to make an Estimate, with what Force the Heart of this Mare must propel the Blood; to raise it in the Tube to the Height of nine Feet six Inches, I injected the right Ventricle of the Heart in the following manner, *viz.*

11. I fixed a musket Barrel to the pulmonary Vein, near its Entrance into the left Auricle; and tyed the ascending and descending *Aorta's* fast, at some Distance from their branching off from each other: Then placing the Barrel in a perpendicular Posture, with a Funnel on the top of it, I poured in melted Bees-wax, till the Funnel was half filled. Yet as I had found by Experience, this perpendicular Height of melted Wax, which was near four Feet, would not have filled the Auricle and Ventricle, if I had not at the same time taken care to pass a small brass Pipe, thro' one of the ascending Branches of the *Aorta*, into the left Ventricle; thro' which the Air passed off as the Wax entered into the Ventricle, the brass Pipe being at the same time gradually drawn up by

C

an

an Assistant, who as soon as all the Air was driven out tyed that Branch of the *Aorta* to prevent the flowing out of the Wax.

12. I chose this Method of injecting from a perpendicular Height rather than by a Syringe, both because I was by this means assured of the Force with which the injected Cavity of the Heart was dilated, which is more uncertain with a Syringe; and also because this dilating Force from the perpendicular Height, continued acting uniformly till the Wax was grown stiff and hard.

13. When cutting open the left Ventricle, I found the Thickness of its muscular Coat to be $1 + \frac{1}{2}$ Inch, and the thinnest Part of that of the right Ventricle was half an Inch.

14. Then taking out the Wax which was formed in the Shape of the Ventricle, I cut the Wax of the left Ventricle off, where the Valves called *Mitrales*, made the Separation; which Valves were propell'd inward by the entering Wax; and I did the same also at the Orifice of the *Aorta*, where the Valves called *Semilunares* were also propelled inward by the abovementioned brass Vent-Pipe.

15. And this is the proper Cavity of the left Ventricle, just before its Contraction; for at
that

that instant, the Blood flowing in from the Auricle has opened the mitral Valves inward, while at the same time the contracting Arteries repel the Blood forcibly against the semilunar Valves; but at the Instant that the Ventricle contracts, the mitral Valves are closed, being expelled by the Blood outwards, while at the same time the semilunar are by the same Action opened outwards, to make way for the compressed Blood to rush into the *Aorta*.

16. So that this Piece of Wax thus formed, may reasonably be taken to be nearly commensurate to the Quantity of Blood received into this Ventricle at each *Diafiole*, and is thence propelled into the *Aorta* at the subsequent *Systoles*.

17. Having therefore filled a narrow mouthed Vessel brim-full of Water, I immersed the Wax in it, then taking it out of the Water, I filled the Vessel brim-full again, from another Vessel whose Capacity was divided into cubic Inches, which gave the Bulk of the Wax, and consequently the Capacity of the left Ventricle, equal to ten cubic Inches.

18. I got the Quantity of the Surface of the Sides of this Ventricle by laying Pieces

of Paper aptly cut to the irregular Form of the several Parts of the Wax; and then laying those Papers under another Paper which was equally divided into little Squares of $\frac{1}{4}$ Inch each; by running a Pin thro' both Papers at every Corner of each Square, the under Papers being thus marked too, it was easy by numbering their several Squares and Parts of a Square, to come pretty nearly to an Estimate of the whole inward Surface of the Ventricle: Which I by this means found to be equal to 26 square Inches, deducting one square Inch for the Area of the Orifice of the *Aorta*, whose Diameter I measured from the injected Wax.

19. The Diameter of the *Aorta* just before the coronary Artery branches from it, was 1.15 Inch.

Whence its Area 1.036 square Inch.

The Diameter of the descending *Aorta* 0.93, its Area 0.677.

The Diameter of the ascending *Aorta* 0.74, its Area 0.369.

20. The inward Area of the Sides of the left Ventricle being therefore equal to 26 Square Inches; the Sum of the whole Pressure of the Blood against all the Sides of that Ventricle,

Ventricle, at the Instant when it begins first to contract, so as to sustain the Pressure of the arterial Blood, will be that Surface or Area multiplied into the perpendicular Height of the Blood in the glass Tube, *viz.* 26×114 Inches, *viz.* 2964 cubic Inches of Blood.

21. And a cubic Inch of Blood weighing 267.7 Grains, these multiplied into 2964 the Number of cubic Inches, and then reduced into Pounds gives 113.22 Pounds, which is the Sum of the Pressure of the Blood, which this Ventricle sustains, at the Instant when it is going to exert a contractive Force, sufficient to propel it with considerable Velocity into the *Aorta*.

22. The Scruple Avoirdupoise contains 18.25 Grains, the Ounce 238 Grains, the Pound 7008 Grains.

23. The Area of the greatest Section of this Ventricle from *Apex* to Base being 6.83 square Inches; these multiplied into 114 Inches, the perpendicular Height of the Blood in the Tube, give 778.63 cubic Inches of Blood, equal to 29.7 Pounds; the Force of the Blood which the muscular Fibres in that transverse Section of the Ventricle must resist.

24. The Velocity with which the Blood is

thrown out of the Ventricle into the Orifice of the *Aorta* may be thus computed, *viz.* the Capacity of this Ventricle being equal to ten cubic Inches, and the Area of the transverse Section of the *Aorta* being 1.036, by which dividing the ten cubic Inches, the Quotient 9.64 is the Length of the Cylinder of Blood, which is formed in passing thro' the *Aorta's* Orifice, at each *Systole* of the Ventricle. And a Horse's Ventricle of his Heart contracting or his Pulse beating 36 times in a Minute, that is 2160 times in an Hour, then a Column of Blood so many times 9.64 Inches, or 20819.4 Inches long or 1734.9 Feet will pass in an Hour.

25. But the *Systoles* of the Ventricle during which that Quantity of Blood is propell'd, being estimated to be done in one third of the Space of time between each Pulse, the Velocity of the Blood during each *Systole* will be thrice as much, *viz.* at the Rate of 5204.7 Feet, *i. e.* 0.98 of a Mile in an Hour or 86.7 Feet in a Minute.

26. Now this Velocity is only the Velocity of the Blood at its first entering into the *Aorta*, in the Time of the *Systole*; in consequence of which the Blood in the Arteries, being

being forcibly propelled forward, with an accelerated *Impetus*, thereby dilates the Canal of the Arteries, which begin again to contract at the Instant the *Systole* ceases : By which curious Artifice of Nature, the Blood is carried on in the finer Capillaries, with an almost even Tenor of Velocity, in the same manner as the spouting Water of some fire-Engines, is contrived to flow with a more even Velocity, notwithstanding the alternate *Systoles* and *Diaستoles* of the rising and falling *Embolus* or Force ; and this by the means of a large inverted Globe, wherein the compressed Air alternately dilating or contracting, in Conformity to the workings to and fro of the *Embolus*, and thereby impelling the Water more equably than the *Embolus* alone would do, pushes it out in a more nearly equal Spout.

27 And since the Blood in the finest capillary Arteries, presses into the Veins with a much more equable Velocity, than in the *Aorta* and greater Arteries ; since also the *Systole* is supposed to be nearly one third of the Time between Pulse and Pulse, the other two thirds of that Time must be spent in the Contraction of the Arteries : it may therefore reasonably be concluded, that the Sum of the

Dilatation of all the Arteries in each *Systole* is equal to about the Quantity of two thirds of the Blood, which is thrown out in each *Systole*, which in the Case of this Mare is equal to two thirds of ten cubic Inches, *viz.* 6.66.

28. This Ventricle throwing out ten cubic Inches at a time, will in the thirty six Pulses of a Minute throw out 360 cubic Inches, equal to 13.75 Pounds, and in an Hour 825 Pounds weight of Blood, nearly equal to the Weight of the Horse.

29. The Area of the transverse Section of the *Aorta* being as above noted 1.036 Inch: And the immediate next Divisions of it being in the Area of the like Section of the descending *Aorta* 0.677 Inch; and that of the ascending *Aorta* being 0.369, we find the Sum of the two Areas of these ascending and descending Branches is greater than that of the Trunk they arise from, and accordingly the Velocity of the Blood will be proportionably abated in them; as also on account of what passes through the coronary Arteries, before the Blood arrives at those two Branches, of which the descending *Aorta* is considerably the largest, thereby to furnish a greater Quantity of
Blood

Blood, in the Proportion that all the Parts of the Body below the Heart, exceed the Bulk of those above the Heart.

EXPERIMENT IV.

1. **I**njected also with Wax the left Auricle and Ventricle of an Ox's Heart, which Ox was by guess supposed to weigh about 1600 Pounds when alive.

The Capacity of this Ventricle was equal to 12.5. cubic Inches.

The Area of the transverse Section of the *Aorta* equal to 1.539 Inch.

That of the descending *Aorta*, equal to 0.912, that of the ascending equal to 0.85.

2. The Pulse of a very gentle Cow, which was not terrified nor disturbed, while its Pulse was counted, was at the rate of thirty eight in a Minute nearly the same as that of a Horse.

3. The Capacity of this Ventricle 12.5 being divided by the Area of the Orifice of the *Aorta* 1.539, the Quotient 8.1. Inches is the length of the Cylinder of Blood, which is formed in passing through the *Aorta* in each *Systole* of the Ventricle.

4. And an Ox's Pulse beating, or this Ventricle contracting 38 times in a Minute, that is 2280 times in an Hour, then a Column so many times 8.1 inches, or 18468 Inches long, or 1539 Feet will pass in an Hour.

5. But each *Systole* of the Ventricle being performed in one third of that time, the Velocity of the Blood in each *Systole* will be thrice as great, *viz.* 4617 Feet, *i. e.* 0.874 of a Mile in an Hour, or 76.95 Feet in a Minute.

6. This Ventricle throwing out 12.5 cubic Inches at a time, will in thirty eight *Systoles*, which it performs in a Minute, throw out 18.14 Pounds, and in an Hour and twenty eight Minutes, it will have thrown out 1600 Pounds of Blood, a Quantity equal to the Weight of the Ox. But this Ox being fat a Quantity of Blood equal to his Weight must be longer in passing through its Heart, than in the lean Horse, *Exper. III. Numb. 27.* for the Fat of Animals has little or no Blood in it, whence lean Animals have *cæteris paribus* much more Blood in them than fat ones.

EXPERIMENT V.

1. I took an Estimate also of the Force of the Blood in a fat gelt Sheep or Wether, by fixing Glass Tubes to the jugular Vein and carotid Artery, in the same manner as I had done to the Horse in *Exper.* III. The Sheep was three Years old, and weighed ninety one Pounds alive.

2. Its Pulses beat sixty five times in a Minute.

3. The Blood rose in the Tube fixed to the jugular Vein $5 + \frac{1}{2}$ Inches, and nine Inches when the Sheep struggled and strained.

4. In the Tube fixed to the carotid Artery it rose six Feet $5 + \frac{1}{2}$ Inches.

5. The Capacity of the left Ventricle of its Heart, was equal to 1.85 cubic Inch.

6. Its inward Surface = 12. 35. square Inches.

7. Its greatest transverse Section = 2.54.

8. The Area of the transverse Section of the Aorta = 0.172. square Inch, that of the descending *Aorta* = 0.094, that of the left carotid Artery = 0.012, and of the right = 0.07, they both rose separate immediately from the *Aorta*.

9. The

9. The inward Surface of this left Ventricle being equal to 12 square Inches; this multiplied by six Feet $5\frac{1}{2}$ Inches produces 930 cubic Inches of Blood = 35.62 Pounds, the Weight of Blood which this Ventricle sustains, just before its *Systole* begins.

10. And the Area of its greatest transverse Section being = 2.54 square Inches, this multiplied into six Feet $5\frac{1}{2}$ Inches the height of the Blood in the Tube, the Product is 393.7 cubic Inches of Blood = 15.03 Pounds, the weight of Blood which the Fibres in this transverse Section of the Ventricle must sustain.

11. The Capacity of the left Ventricle being = 1.85 cubic Inch, which divided by 0.172, the Area of the transverse Section of the *Aorta*, the Quotient 10.75 is the length of the Cylinder of Blood, which is formed in passing through the *Aorta* in each *Systole* of the Ventricle.

12. And this Sheep's Pulse beating, or his left Ventricle contracting sixty five times in a Minute, that is 3900 times in an Hour, therefore a Column of Blood so many times 10.75 Inches, or 41875 Inches long, or 3489.5 Feet will pass in an Hour.

13 But

13. But the *Systoles* of the Heart, during which that Quantity of Blood is propelled, being estimated to be done in one third of the Space of Time between each Pulse, the Velocity of the Blood during each *Systole* will be thrice as much, *viz.* at the Rate of 10468.5 Feet, *i. e.* 1.98 Mile in an Hour, or 174.4 Feet in a Minute.

14. And the Ventricle throwing out 1.85 cubic Inch of Blood each Time, that will be 4.593 Pounds in a Minute, or ninety three Pounds, a Quantity equal to the Weight of the Sheep in twenty Minutes.

EXPERIMENT VI.

1. **H**AVING fixed a Tube to the left crural Artery of a fallow Doe, the Blood rose four Feet two Inches in the Tube.

2. I injected with Wax both Auricles and Ventricles of the Heart of another Doe, and found the Capacity of the Ventricle equal to nine cubic Inches, and the right Auricle and Ventricle near as big.

3. Timorous Animals are observed to have larger Hearts than courageous ones; as Deer,
 Asses,

Asses, Hares, &c. which holds true in the Instance of this Doe's Heart. *Qu.* May not one Reason of this be, that the Fibres of the timorous are generally more lax than those of courageous Animals, on which Account the Blood passing with less Resistance through the lax fibred capillary Vessels, it was requisite that the Heart should at each Pulse throw out a greater Quantity of Blood, in order to supply its more easy and plentiful Flow through the lax capillary Arteries into the Veins? And may not this be the Reason why the Pulses of young Animals, as of Children, are found to beat faster than those of grown Persons, *viz.* because the tender Fibres of the Coats of their Blood Vessels being very lax, they give the less Resistance to the flowing Blood; whose Globules are observed by *Leewenhoeck* to be all of a Size both in great and small Animals; whence it was needful to make Provision for a proportionably greater Supply of it from the Heart, by increasing the Velocity of the Dilatations and Contractions of that curious Engine; in the Formation of which are seen such evident Marks of the consummate Wisdom of the great Author of Nature.

4. The

4. The Area of the transverse Section of the *Aorta* of this Doe $= 0.476$ of the descending *Aorta* $= 0.383$, of the ascending $= 0.246$, and that of the pulmonary Artery $= 0.502$. But it being not easy to obtain, in that timorous Creature, the just Number of Pulses in a Minute; I could not calculate the Velocity of the Blood, nor the Quantity that passes in any determinate time.

EXPERIMENT VII.

1. **I** Fixed Tubes also in the same manner to the jugular Vein and carotid Artery of several Dogs; for whatever Experiment I principally intended to make on any Dog, I usually began with fixing a Tube first to the jugular Vein, and then to the carotid Artery, which was the Method I used to wash the Blood out of the capillary Vessels, thereby the better to prepare them for my intended Experiments.

2. The Force of the Blood in the Veins and Arteries is very different, not only in Animals of different Species, but also in Animals of the same kind, and that not only in those of different Sizes and Weights, but also in Dogs of the same Size and Weight; and
even

even in the same Animal the Force of the Blood in its Vessels, is continually varying, according to the different Kinds and Quantities of Food, the various Distances of time after taking Food, the more or less plethoric State of the Blood Vessels, also from Exercise, Rest, different States of Vigour or Vivacity of the Animal, and many other Circumstances, which may conduce to vary the Force of the Blood: For the healthy State of Animals is not confined to the scanty Limits, of one determinate Degree of vital Vigour in the Blood: But the Allwise Framers of these admirable Machines has so ordered it, as that their healthy State shall not be disturbed by every little Variation of this Force, but has made it consistent with a very considerable Latitude in the Variation of it. Now since this Force of the Blood is so variable, it is the more requisite to be furnished with a good Quantity of Observations, thereby to find out the more nearly, a Medium of those Forces, not only in the same Animal, but also in those of different Ages, Sizes and Kinds; whence haply some curious Observations may arise.

3. These great Inequalities of the Force of the Blood, not only in different Animals, but
also

also in Animals of the same, kind may be seen in the following Table in *Exper. VIII. Numb. 12.* in which I have set down the Weights of most of them, and also in different Columns, the Height to which the Blood rose in Tubes fixed to the Veins and Arteries.

4. I observed here, as in the abovementioned Horses, that when the Blood had subsided a little in the Tubes which were fixed to the Arteries of these Dogs, it would, as in the Horses, rise on a sudden considerably on deep sighing; as also on pressing the Dogs Bellies hard with the Hand, the Blood would immediately rise about six Inches, and subside as much, on taking off the Hand, and it was the same on several Repetitions.

5. It may be objected to this Method of estimating the Force of the Blood, that by thus fixing Tubes to these large Veins and Arteries, the Course of a considerable Stream of Blood was for that time stopped; and that consequently the Force of the Blood must be proportionably increased in all the Veins or Arteries; and therefore also in the Veins or Arteries to which the Tube is fixed. And doubtless in some degree it is so. In the Sheep the left Carotide is nearly $\frac{1}{3}$ part of the right Carotide

tide and descending *Aorta* taken together; and in the Dog, Numb. 3. it is about $\frac{1}{4}$ of them.

6. To obviate therefore this Inconvenience, I fixed Tubes laterally to the jugular Veins and Arteries of the Dog, Numb. 13. in the following manner, *viz.* I took two cylindrical Sticks which were $\frac{1}{2}$ Inch Diameter, and $1 + \frac{1}{2}$ Inch in length; and having bored Holes through them from end to end, something larger than those Veins and Arteries; I then slit them in halves Length-ways, and bored another Hole thro' the middle of one of them into its Cavity, into which lateral Hole the brass Pipe entered; which was at its other End adapted to fit another Pipe which was cemented to a glass Tube. Then having laid the Vein or Artery bare, I drew a linen Cloth under it to wipe it very dry; and then placed under it one of the above-mentioned slit Pieces of Wood, laying the Vein or Artery in its Cavity, which was covered with Pitch, that was at that instant afresh melted with a small warm iron Rod; then pouring melted Pitch not very hot, over the Vein or Artery, I immediately put on the other half of the split Wood, which had the Hole

I

bored

bored thro' it, and tied them fast together : Then entering the very slender Point of a Penknife into the abovementioned Hole, I cut an Orifice in the Vein or Artery, and then immediately fixed the brass Pipe and Tube to receive the following Blood which rose from the jugular Vein of the thirteenth Dog first six Inches, and on straining $9\frac{1}{2}$ Inches, and from the Artery four Feet eleven Inches, and would doubtless have mounted higher, if the Blood had not made an Outlet between the Artery and the Pitch, so as to prevent its Rise ; which Inconvenience might easily be prevented by proper Care ; which if done would give us the real Force of the Blood against the Sides of the Arteries, as it did in this jugular Vein.

7. I believe this would be a good Method to take the Force of the Blood in lesser Animals, where by reason of the Smallness of those Vessels it might be difficult to insert Pipes into them, which if done those Pipes would have too small a Bore for the Blood freely to pass thro' them.

8. I have noted in the following Table, *Exper. VIII. Numb. 12.* the several Heights to which the Blood rose in Tubes fixed to

the Veins and Arteries of Animals, as they lay horizontally on their Backs, or on one side in the Case of the Mare, *Exper.* III. But when an Animal stands on its Legs, a Column equal to the perpendicular Height of the Animal, must be added to the several Heights of the Blood in the glass Tubes, in order to estimate the Force, with which the Blood presses against the Coats of the Blood-Vessels, at the lower Parts of the Body, and so in proportion for any other Part that is higher. So that these Columns of Blood in the Arteries and Veins, communicating with each other below, are on account of their equal Heights, equipollent to each other : The progressive Motion of them being determined by the Energy of the Heart. And tho' Valves in Tubes in which a Fluid is propelled upwards with an equable Force, would rather retard than promote its Progress; yet in Tubes where the Fluid does not ascend equably, but by Reason of frequent Motions of the whole Machine, it is subject to many Agitations; in this case Valves are of great Importance to check the Repercussion and Regurgitation of the Fluid: And accordingly the allwise Framer of animal Bodies has provided Valves in the

I Veins,

Veins, to prevent this Inconvenience, and that principally in the lower Parts of the Body, where they are most needed, especially in great Motions, and in exerting the muscular Force of the Body.

EXPERIMENT VIII.

1. **T**HE Blood having risen six Feet eight Inches from the crural Artery of the Dog, *Numb.* 1. and to the same Height from the left carotide Artery of *Numb.* 7. in the Table, *Numb.* 12. of this *Experiment* VIII. I chose to calculate the Velocity, &c. of the Blood of this Dog.

2. The Capacity of the left Ventricle of the Heart being injected with Wax was found equal to 1.172 cubic Inch.

3. Its inward Surface equal to eleven square Inches, which multiplied into the perpendicular Height of the Blood in the glass Tube, which was fixed to the Artery, *viz.* six Feet eight Inches or eighty Inches, gives eight hundred and eighty cubic Inches of Blood, which press on all Sides of that Ventricle, when it has contracted just so far, as to sustain and be equal to the Force of the Blood in the *Aorta*.

D 3

4. These

4. These eight hundred and eighty cubic Inches multiplied by 267.7 the Number of Grains in a cubic Inch of Blood gives 235576 = 33.61 Pounds.

5. The Area of the transverse Section of the *Aorta*, just before the coronary Arteries branch off from it, being 0.196 square Inch, by which dividing 1.172 cubic Inch, the Capacity of the Ventricle, the Quotient 5.978 Inches is the Length of the Cylinder of Blood, which is formed in passing thro' the Orifice of the *Aorta*, at each *Systole* of the Ventricle.

6. And a Dog's Pulse being found to beat, or his left Ventricle to contract ninety seven times in a Minute, then a Column of Blood so many times 5.97 Inches long, will be 34745.4 Inches or 2895.45 Feet long: But the *Systoles* of the Heart during which that Quantity is propelled, being estimated to be done in one third of the Time between Pulse and Pulse, the Velocity of the Blood during each *Systole* will be thrice as much, viz. 8586.35 Feet, that is at the rate of 1.62 Mile in an Hour, or 143.1 Feet in a Minute.

7. And the Ventricle throwing out 1.172 cubic Inch of Blood in each *Systole*, that is 4.34 Pounds in ninety seven Pulses, the Num-

ber

ber of Pulses in one Minute, hence fifty two Pounds, a Quantity equal to the Dog's Weight, will pass thro' the Heart in 11.9 Minutes.

8. If according to Dr. Keill's Estimate the left Ventricle of a Man's Heart throw out in each *Systole* an Ounce or 1.659 cubic Inch of Blood, and the Area of the Orifice of the *Aorta* be $=0.4187$; then dividing the former by this, the Quotient 3.96 is the Length of the Cylinder of Blood, which is formed in passing thro' the *Aorta* in each *Systole* of the Ventricle; and in the seventy five Pulses of a Minute a Cylinder of 297 Inches Length will pass this at the Rate of 1493 Feet in an Hour. But the *Systole* of the Heart being performed in one third of this Time, the Velocity of the Blood in that instant will be thrice as much, viz. at the rate of 4479 Feet in an Hour, or 74.6 Feet in a Minute.

9. And if the Ventricle throws out 1.172 cubic Inch in a Pulse, then in the seventy five Pulses of a Minute the Quantity of Blood will be equal to 4.37 Pounds, and in 36.3 Minutes a Quantity equal to a middle-sized Man, viz. a hundred and sixty Pounds will pass thro' the Heart.

10. But if with Dr. *Harvey* and Dr. *Lower* we suppose two Ounces of Blood, that is 3.318 cubic Inches to be thrown out at each *Systole* of the Ventricle, then the Velocity of the Blood in entering the Orifice of the *Aorta* will be double the former, *viz.* at the rate of 149.2 Feet in a Minute, and a Quantity of Blood equal to the Weight of a Man's Body will pass in half the Time, *viz.* 18.15 Minutes.

11. If we suppose, what is probable, that the Blood would rise $7 + \frac{1}{2}$ Feet high in a Tube fixed to the carotide Artery of a Man, and that the inward Area of the left Ventricle of his Heart, is equal to fifteen square Inches, these multiplied into $7 + \frac{1}{2}$ Feet give 1350 cubic Inches of Blood which presses on that Ventricle, when first it begins to contract, a Weight equal to 51.5 Pounds.

12. That we may the more readily compare the abovementioned several Estimates together, I shall here range them in order in a Table.

1900

The Several Animals.	Weight of each.	Height of the Blood in the Tube from Ju- gul.	Height of the Blood in Tubes fixed to Arteries.	Capacity of the left Ven- tricle of the Heart.	Area of the Ori- fice of the Aor- ta.	Velocity of the Blood in the Aor- ta.
	Pd. Ou.	Inches.	Feet Inch	Cub. Inches.	Square Inches.	Feet Inch in a Mi- nute.
Man	160	On straining.	7 6	1.659	0.4187	74.6
				3.318		149.2
Horse 1st			8 3			
2d			9 8			
3d	825	12 52	9 6	10.	1.036	86.7
Ox	1600			12.5	1.539	76.95
Sheep	91	5 $\frac{1}{2}$ 9	6 5 $\frac{1}{2}$	1.85	0.172	174.4
Doe			4 2	9	0.476	
Dogs 1st	52	0 6	6 8	1.172	0.196	143.1
2d	24	5 7	2 8	1	0.185	130.9
3d	18	5	4 8	0.633	0.118	127.4
4	12 8	4	3 3	0.5	0.101	120
5		4 6	at crural	1.25	0.210	143
6	31		Arter.		0.196	
7	43		6 8	1.172	0.179	156.5
8			6 6	Tube fixed to the crural Artery.		
9		7 14	3 1	was very old and died soon.		
10	15	5 24	1 6			
11	37	8 $\frac{1}{2}$	4 9	Tube fixed laterally to the left carotide Artery.		
12	36		6 7			
13	24	6 9 $\frac{1}{2}$	4 11			
14	37 8		5 8			
15		5 19	on sucking at the Tube.			
16		5 $\frac{1}{2}$ 8	on sucking			
17	19	5 14	5 2			
18	35	5	4 7			
19	32	6 9 $\frac{1}{2}$	3 11			
20	23	5 7	4 10			

The several Animals.	Quantities of Blood = to the Weight of the Animal in what Time.	How much in a Minute.	Weight of the Blood sustain'd by the left Ventricle contracting.	Number of Pulses in a Minute.	Area of the transverse Section of descending Aorta.	Area of the trans. Section of ascending Aorta.
	Minutes	Pounds	Pounds		Square Inches	Square Inches
Man	36.3 18.15	4.37 8.74	51.5	75		
Horse	60	13.75	113.22	36	0.677	0.369
Ox	88	18.14		38	0.912	0.85 Ri. left
Sheep	20	4.593	35.52	65	0.094 0.383	0.07 0.012 0.246 Ri. left
Dog	1 11.9 2 6.48 3 7.8 4 6.2	434 3.7 2.3 1.85	33.61 19.8 11.1	97	0.106 0.102 0.07 0.061 0.119 0.125 0.109	0.041 0.034 0.031 0.009 0.022 0.009 0.015 0.007 0.7 0.031 0.062 0.031 0.053 0.032
	7 6.56	4.19				

13. I do not see, by comparing the Weights of these Animals, and the several Quantities of Blood which pass thro' their Hearts in a given Time, that we can thence form any Rule that is fixed, for the proportioning the Quantities of flowing Blood to their different Sizes.

14. These Quantities in larger Animals are very disproportionate to the Bulk of their Bodies, in comparison of what they are in lesser Animals, as estimated in this Table.

15. But as in the bigger Animals the Blood has a longer Course to go, and must therefore meet with a greater Resistance ; so we may observe in this Table, by comparing the perpendicular Heights of the Blood in the Tubes fixed to the Arteries, that the Force of it in the Arteries, is in the main greatest in the largest Animals.

16. And supposing the Blood-vessels in the Man and Horse to be equally distributed in all their homologous Parts, that is if they are proportionable to their respective Weights, then the Blood must move in them reciprocally as the Times, in which Quantities of Blood equal to their respective Weights, pass thro' their Hearts, *viz.* as 60 to 15.15 Minutes.

17. So that notwithstanding the arterial Blood of a Horse is propelled with a greater Force than that of a Man ; yet it moves the slower in the Horse on account of a greater Number of Ramifications, and a greater Length of Vessels in the larger Animal.

18. When I compared the Proportion, that the Area of the transverse Sections of the descending *Aorta* bear to the Flesh and other Parts which they supply with circulating Blood, I found it to be as follows, *viz.* having cut the Body of a Dog asunder at his Heart, and first weighing each part separately, and then boiling them so as to separate the Bones from the Flesh ; the Weight of the Bones being deducted from the Flesh, the Flesh, &c. of the hinder part was found to weigh eleven Pounds eleven Ounces; that of the fore-part seven Pounds two Ounces.

19. Now the Areas of the transverse Section of the Arteries of these five Animals are by Measurement as follow, *viz.*

20. In

	Aorta. desc. ascend.				
20. In the Mare	—1.036—	0.677—	0.369	by computation	0.412
Ox	—1.539—	0.912—	0.85	from the above	0.056
Sheep	—0.172—	0.094—	0.082	found Propor-	0.057
Doe	—0.476—	0.383—	0.246	tion of Flesh in	0.234
1 st Dog	—0.196—	0.106—	0.075	the hinder and	} 0.065
6 th Dog	—0.196—	0.125—	0.093	fore parts.	
7 th Dog	—0.179—	0.109—	0.085		

21. In this Table we find that the Areas of the transverse Section of the descending and ascending *Aorta's* of the first Dog are nearly proportionable to the Weights of the respective Parts which they supply with Blood; and that in the Mare and Doe the Difference is not great, but greater in the Ox and Sheep. In Estimates of this kind great Accuracy in the Proportions is not to be expected.

22. The Velocity with which the Blood is thrown out of the left Ventricle, being performed in one third of the time between *Systole* and *Systole*, the like Quantity of Blood would move with an equable Motion, of one third of that Velocity, thro' the Orifice of the *Aorta* in the Space of Time between each *Systole*.

23. Since in a Man a Cylinder of Blood of the Diameter of the Orifice of the *Aorta* and 7.92 Inches long, is at each Pulse impelled thro' a dilatable conical Artery, its Velocity

Velocity would be greatly increased, by passing thro' that narrower Defilee; but the Arteries continually sending off innumerable Branches, the Sum of whose Orifices are considerably larger than the main Stems, hence the Velocity of the Blood must be proportionably rebated. So that as Dr. *James Keill* in his *Tentamina Medico-Physica*, p. 46. has estimated it, the Velocity of the Blood at the Heart, would be to its Velocity in an evanescent Artery as 5233 to 1, if it had a free unimbarressed Course thro' those capillary Arteries. And since the Velocity at its passing from the Heart to the *Aorta* is at the rate of 149.2 Feet in a Minute, taking one third of that, *viz.* 49.73 for its continued equable Velocity, according to Dr. *Keill's* Estimate it would move but 0.0095th part of a Foot or 0.083 Inch in a Minute, in the evanescent Arteries in that time.

24. This would be its Velocity there, if the Blood had as free and unimbarressed a Course thro' the finer capillary Arteries, as it has thro' their larger Ramifications. But by the following Experiment it is found, that the principal Obstruction to the Progress of the arterial Blood is in the capillary Arteries.

EXPERI-

E X P E R I M E N T IX.

1. **I** Slit open with a Pair of Scissors, from end to end, the Guts of a Dog, on that side which was opposite to the Insertion of the mesenterick Arteries and Veins ; and having fixed a Tube 4 $\frac{1}{2}$ Feet high to the descending *Aorta* a little below the Heart, I poured blood warm Water thro' a Funnel into the Tube, which descended thence into the *Aorta*, with a Force equal to that, with which the Blood is there impelled by the Heart : This Water passed off thro' the Orifices of innumerable small capillary Vessels, which were cut asunder thro' the whole Length of the slit Gut. But notwithstanding it was impelled with a Force equal to that of the arterial Blood in a live Dog, yet it did not spout out in little distinct Streams, but only seemed to ouze out at the very fine Orifices of the Arteries, in the same manner as the Blood does from the capillary Arteries of a Muscle cut transversely.

2. Having provided a Pendulum which beat Seconds, and pouring in thro' the Tube known Quantities of warm Water, I found
that

that 342 cubic Inches of Water passed off in 400 Seconds or 6.6 Minutes.

3. Then cutting all the mesenteric Arteries asunder close to the Guts, and taking away the Guts, I found that a like Quantity of Water passed thro' these larger Ramifications of the Arteries in 140 Seconds, or 2.3 Minutes, that is in one third of the Time.

4. Then cutting asunder the crural Arteries, which were before tyed, and cutting off the mesenteric and emulgent Arteries close to the *Aorta*, a like Quantity of Water passed thro' this thus cut *Aorta* in 0.308 Minute, that is in $\frac{1}{21.4}$ th part of the Time, in which it passed thro' the capillary Arteries of the slit Guts.

5. Therebeing 342 cubic Inches which passed thro' the capillary Arteries of the slit Guts in 6.6 Minutes, that is thirteen Pounds if it were Blood, or 1.939 Pound in a Minute: And it being estimated in the Table, *p.* 43. that 4.34 Pounds of Blood were thrown out of the Heart of the Dog, *Numb.* 1. in a Minute, the abovementioned 1.939 Pound is $\frac{1}{6.44}$ th Part of what passes the Heart in that Time.

6. But on weighing all the fleshy and other membranous Parts of another Dog, thro' which the arterial Blood passes, that is, exclusive of the Bones and Lungs, I found the whole Weight to be eighteen Pounds eleven Ounces, of which the slit Gut weighing one Pound two Ounces was therefore $\frac{1}{16.6}$ th part of the whole: So that Bulk for Bulk there passed 30.27 times more Water thro' the Arteries of these slit Guts than thro' the rest of the Arteries of the Body, and that with a Force no greater than that of the Heart.

7. Which may reasonably be attributed to these several Causes, as to the much greater Fluidity of Water than of viscid Blood, to the more relaxed State of these Arteries in the dead than in the living Animal; for tho' the Arteries and Veins of a dead Animal being then freed from the distending Force of the Blood do contract, yet with equal Forces, those of the dead Animal will dilate more than those of a live Animal: But this more plentiful Flow of Water is principally owing to the great Difference there is in Size, between these cut capillary Arteries, and the succeeding Series's of exceeding small Ramifications, and that at right Angles, thro' which the Blood passes
in

in its further Progress towards the Veins: As also to a want of the Resistance of the venal Blood, which rising six Inches in the Tube fixed to the jugular Vein is $= \frac{1}{13.33}$ part of the Force of the arterial Blood, and must therefore proportionably retard its Motion.

8. The Diameters of the cut Orifices of the Arteries thro' which the Water passed off, were at a Medium, one with another, equal to twice the Diameter of a Hair, which Dr. *Jurin* by an accurate Estimation found to be $\frac{1}{324}$ th part of an Inch; hence these Arteries which are $\frac{1}{162}$ Inch Diameter, as they branch off from the Mesenterics spread themselves alternately, on each side of the Guts, whence meeting again, their inosculating Branches form *Areola's* like those that are on the Leaves of Trees: And from these thus converging Arteries and sap Vessels, there branch off nearly at right Angles, without converging any more, much smaller Arteries, and from these others again, both at right Angles, and like the spread Fingers of a Hand, in successive Series's in their Progress towards the Veins.

9. The Diameters of the first Series's of these unconverging Branches, may in a Piece of Gut well injected with Vermilion, be ob-

served to be of several Sizes from $\frac{1}{2}$ to $\frac{1}{3}$ of the Arteries whence they rose, and the succeeding ones finer and finer to nearly $\frac{1}{3240}$ th part of an Inch, that is so fine, that only single Blood globules can pass them into the Veins; here therefore so viscid a Fluid as the Blood must needs meet with a very great Resistance.

10. If we compare the Area of the Orifice of the brass Pipe, which was in this Experiment fixed to the descending *Aorta*, with the Sum of the Areas of all the large mesenteric Arteries just before they subdivide into Branches, which immediately enter the Coats of the Guts, and also with some of the Areas of the cut Orifices of the above-mentioned converging Arteries, they are as follow, *viz.*

11. The Area of the Orifice of the brass Pipe was 0.057 square Inch, taking a mean Diameter of one of the mesenteric Arteries to be 0.06 Inch, its Area 0.0028, and counting 82.8 of these in $11\frac{1}{2}$ Feet Length of the slit Gut, the Sum of all their Areas will be 0.234 Inch.

12. Taking also the Diameter of one of these Arteries at their first entering the Guts to be equal to 0.02, its Area will be 0.000314, and the Sum of 724.25 of these Areas in $11\frac{1}{2}$ Feet length of Gut will be 0.227.

14. And

13. And a mean Diameter of a converging Artery at the slit part of the Gut being $\frac{1}{162}$ Inch equal to 0.00672, its Area will be 0.000135, and the Number of all these Arteries in $11 \frac{1}{2}$ Feet length of Gut being 1695, the Sum of the Areas of all their Orifices at the slit part will be 0.2288 square Inch.

14. If we take a small converging Artery whose Diameter is $\frac{1}{324}$ equal to 0.000308, its Area will be 0.00000744; I have observed from each side of such an Artery, four Branches to go off at right Angles, each of whose Diameter was half of the converging Artery, *viz.* $\frac{1}{648}$ Inch equal to 0.00154 whose Area is 0.00000218, which multiplied by 8, the Number of those Branches, the Sum of their Areas is 0.00001744, which exceeds the former by 0.00001.

15. These reticular converging Arteries, by being thus inosculated into each other, both prevent Obstructions in them, and also thereby the more plentifully supply the next Series of rectangular Branches with Blood; for if the Blood had entered the converging Arteries only at one end, its Velocity would thereby have been more retarded in going the whole Length than half the Length of these converging

ing Arteries ; by these innumerable Convergencies of the Arteries the Blood is more blended and mixed, as is plain to be seen in the Lungs of Frogs.

16. By comparing the Sum of the transverse Section of those several mesenteric and converging Arteries together, we may observe that the first and second Order of the mesenteric Arteries are nearly equal, *viz.* 0.228 0.234. 0.227, yet equal Quantities of Water were found to pass in one third of the time thro' the cut mesenteric Arteries, that they did thro' the cut converging Arteries of the slit Guts, that is since the Quantities which flow thro' equal Tubes are reciprocally as the times, the Quantities in these converging Arteries ought to have been as 981.38 to 342 cubic Inches, whereas there flowed but $\frac{1}{2.86}$ th part.

17. And tho' the Area of the Orifice of the brass Pipe, which was fixed to the *Aorta* was but 0.057 that is but $\frac{1}{4}$ part of the Sum of the other Arteries, yet 1148.9 cubic Inches would have passed that in 6.6 minutes, when the great Branches of the *Aorta* were cut off, that is, 1.17 times as much would in that time flow thro' it as thro' the mesenteric Arteries, and 3.3 times as much, as did pass thro'

thro' the cut Orifices of the converging Arteries.

18. Hence we see how greatly the Velocity of the Water is retarded in passing thro' the several Branchings of the Arteries, notwithstanding the Sum of the Areas of their transverse Section is considerably greater than that of the *Aorta*. And this Retardation must be still greater to the Blood which is both a grosser and more viscid Fluid than Water, and that especially in the extream capillary Arteries which branch off at right Angles, and which are about $\frac{1}{1620}$ th part of an Inch in Diameter, so fine that only single Globules of Blood can pass them.

19. And to this Resistance which the Blood meets with in passing the capillary Arteries, is owing the great Difference of the Force of the Blood in the Arteries to that in the Veins, viz. as 10 or 12 to 1.

20. For tho' the Velocity of the Blood at its first Entrance into the *Aorta*, depends on the Proportion the Area of its Orifice bears to the Quantity thrown into it at each *Systole*, and also on the Number of those *Systoles* in a given time : Yet the real Force of the Blood in the Arteries, depends on the Proportion, which

the Quantity of Blood thrown out of the left Ventricle in a given time, bears to the Quantity which can pass thro' the capillary Arteries into the Veins, in that time.

21. But the Resistance which the Blood meets with in those capillary Passages, may be greatly varied, either by the different Degrees of the Viscidity or Fluidity of the Blood, or by the several Degrees of Constriction or Relaxation of those fine Vessels; Instances of which may be seen in Experiments XV, XVI, XVII, XVIII.

22. And as the State of the Blood or Blood-vessels are in these Respects continually varying from divers Causes, as Motion, Rest, Food, Evacuations, Heat, Cold, &c. so as probably never to be exactly the same, any two Minutes, during the whole Life of an Animal; so nature has wisely provided, that a considerable Variation in these, shall not greatly disturb the healthy State of the Animal.

23. We may make a pretty near Estimate of the Force of the Blood in the capillary Vessels in the following manner, *viz.* taking the Diameter of a Blood Globule to be as above $\frac{1}{3240}$ th part of an Inch; which *Leeuwenhoek* has observed to be of the same Size both
in

in small and great Animals; and allowing these capillary Vessels to be a small Size larger than the Globules, which swim in and are carried along by *Serum* which surrounds them on all sides, we may therefore well suppose one of these Vessels to be double the Diameter of such a Globule, *viz.* $\frac{1}{162}$ th part of an Inch or 0.00617, the Periphery therefore of this Vessel will be 0.0194, and its Area 0.0000298, which multiplied by 80, the Number of Inches to which the Blood rose in the Tube when fixed to the Artery of the Dog Numb. 1, gives 0.0000298 parts of eighty cubic Inches of Blood or of 21416 Grains, equal to 1.997 Grains. But the Resistance of the Blood in the Veins of the same Dog being found equal to six Inches Height or $\frac{1}{12.5}$ th part of eighty Inches, this $\frac{1}{12.5}$ th part being deducted out of 1.997 Grain, the Remainder 1.838 Grain is the Force with which the Blood would be impelled into such a Capillary by a Column of Blood of eighty Inches Height, supposing it were in a stagnant State; to which also must be added the Velocity which the Blood has acquired at its first Entrance in the capillary Vessel, which can be but small as appears by the great Resistance it meets with in the capillary

lary Vessels, in this IXth Experiment, Numb. 18. whence we see both from Experiment and Calculation, that the Force of the Blood in these fine Capillaries can be but very little, and the longer such Capillaries are, the slower will the Motion of the Blood be in them.

24. It is observable that these parallel Arteries are not as in the Bowels, Lungs and other membranous parts of the Body, intermixed with corresponding similar Veins ; but two different Series's of these Arteries, arising at right Angles from larger Arteries, one Series from the upper, and the other from the lower parts of the Muscles, their parallel Arteries do mutually and alternately intermix, whereby the Blood is conveyed in them alternately upwards and downwards, and thence flows at right Angles into the Veins.

25. From this very small Force of the arterial Blood among the muscular Fibres we may with good reason conclude, how short this Force is of producing so great an Effect, as that of muscular Motion, which wonderful and hitherto inexplicable Mystery of Nature, must therefore be owing to some more vigorous and active Energy, whose Force is regulated by the Nerves ; But whether it be confined

finèd in Canals within the Nerves, or acts along their Surfaces like electrical Powers, is not easy to determine.

26. That a vibrating electrical Virtue can be conveyed and freely act with considerable Energy along the Surface of animal Fibres, and therefore on the Nerves, is evident from curious Experiments, made by that skilful and indefatigable Experimenter Mr. *Stephen Gray*, of which he has given an Account in the *Philosophical Transact.* Numb. 417. 422, where he shows that electrical Virtues from a Glass heated by rubbing, will not only be conveyed along the Surface of Lines to very great Lengths, but will also be freely conveyed from the Foot to the extended Hand of a human Body suspended by Ropes in a horizontal Posture in the Air; and also from that Hand to a long Fishing Rod held in it, and thence to a String and a Ball suspended by it: And also that an electrical Virtue may be carried along a Surface of Water.

27. And it has been frequently observed, that when some part of the Body has upon itching been gently scratched by the Nails, there has at the same time been felt in a distant part, a very pungent *Stimulus* or Sensation answering exactly

exactly Stroke for Stroke to the Action of the scratching Nails. Thus particularly on scratching a small Pimple, a little below the right Knee on the outside, a like pungent Sensation has been felt on the left Shoulder Blade, and sometimes on that Arm some Inches below the Shoulder : And *vice versa* the right Shoulder or Arm has in like manner been affected, when the scratching has been made near the left Knee, but this Effect does not always follow : There are many Instances of the Sympathy of the Nerves.

28. That the animal Spirits, whether they act within or on the Outsides of the Nerves, are elastick, seems probable not only from their great Activity and Energy ; but also from the sudden and strong Effects that sulphureous Vapours, which are known to destroy Elasticity, are found by experience to have on them. Thus the Fumes of burning Brimstone will instantly deprive all Animals whatever of Life : Thus also the subtle and most penetrating Fumes of fermenting spirituous Liquors, are known either to strike those instantly dead who smell to them, or to infatuate or render Paralytick for Life, those who smell to them in lesser Degrees. Thus also the sulphureous fœtid Fumes
of

of burnt Feathers, &c. have an Effect on the disordered Spirits of those who are in Fits. Thus also *Assa Fætida*, *Castor*, &c. which abound with a subtile Sulphur, are found to be friendly to the Spirits of the Hysterick; as on the contrary are many Fumes most offensive to the Spirits of others.

29. If the Skin be flead off the Belly of a live Frog, and the *Abdomen* opened on each side, so as that its strait Muscles may by drawing a little on one side, have a strong focal Light cast on the Inside of them; if in this Posture those Muscles be viewed thro' a good Microscope, the parallel Fibres of the Muscles are plain to be seen, with the Blood running alternately up and down, between each Fibre, in capillary Arteries so fine that only single Globule can pass them. If the Muscle happens to act while thus viewed, then the Scene is instantly changed from parallel Fibres, to Series's of *Rhomboidal Pinnulæ* which immediately disappear as soon as the Muscle ceases to act. It is not easy to get a Sight of this most agreeable Scene, because that on the Action of the Muscle, the Object is apt to get out of the *Focus* of the Microscope; but those who are expert in the Use of those Glass-

es may readily move them accordingly. I have found small Frogs best for this Purpose, *viz.* such as are not above $\frac{1}{3}$ or $\frac{1}{4}$ of their full Growth. Stimulating the Foot of a Frog, will sometimes make it contract these Muscles. The Frog must be fixed in a proper Frame. If repeated Observations were made on the Muscles thus in Action, it might perhaps give some farther Insight into the Nature of muscular Motion.

30. It may not be improper here to take notice, that having about twenty seven Years since, read the unsatisfactory Conjectures of several, about the Cause of muscular Motion, it occurred to me, that by fixing Tubes to the Arteries of live Animals, I might find pretty nearly, whether the Blood by its meer hydraulick Energy, could have a sufficient Force, by dilating the Fibres of the acting Muscles, and thereby shortning their Lengths, to produce the great Effects of muscular Motion. And hence it was, as I mentioned in the Preface to the first Vol. that I was insensibly led on from time to time, into this large Field of statical and other Experiments. Whence we see what great Encouragement we have to spur us on in these Pursuits ; since the wonderful

ful Works of the great Author of Nature, are so fruitful in furnishing us, from its inexhaustible Fund, with fresh Matter for our Researches, and thence with the inexpressible Delight, of new and farther and farther Motives to adore and praise our All-glorious Maker in his Works.

EXPERIMENT X.

1. **WE** see in the foregoing Experiment how very much the Velocity of the Blood is retarded, in its Passage from the greater, to the lesser Arteries in the Guts, notwithstanding the Sum of the Diameters and Areas of the lesser do considerably exceed those of the greater. So that the Blood in passing thro' the Muscular, the Membranous, and other parts of the Animal, must be carried on with innumerable different Degrees of Velocity, and consequently in different Quantities thro' dissimilar parts: But in the Lungs especially this Difference is remarkably great, for since the whole Blood of the Animal incessantly passes thro' them; by comparing the Proportion, which the Quantity of the Lungs bears to the rest of the Body, we may make some estimate

estimate of the great Difference of the Velocity of the Blood in each.

2. I cut the Body of a Spaniel Dog asunder just at his Heart, having first taken out the Lungs, the fore part weighed eight Pounds six Ounces, the hinder part twelve Pounds eleven Ounces, the Stomach and Guts being washed clean weighed one Pound two Ounces

3. I boiled the whole Dog so long as to make the Bones easily separate from the Flesh, they weighed two Pounds four Ounces, which being deducted from twenty one Pounds one Ounce, the whole Weight of the Dog; there remains eighteen Pounds thirteen Ounces, for the Weight of the Flesh, Bowels, Skin, Membranes, &c. the Dog was not fat, but allowing six Pounds thirteen Ounces for Fat and Hair, thro' which the Blood does not circulate, there then remains twelve Pounds of Substance, thro' which the Blood may reasonably be supposed to circulate freely.

4. The Windpipe being cut off close to the Lungs, they weighed six Ounces two Drams, that is $\frac{1}{30.72}$ th part of the twelve Pounds.

5. The whole Blood is incessantly circulating, tho' not with equally numerous Revolutions, thro' all the Parts of the twelve Pounds ;
but

but doubtless with vastly greater Rapidity thro' the Lungs, than thro' other Parts of the Body.

6. If according to the Estimate under Experiment VIII, 4.34 Pounds of Blood pass thro' the Heart in a Minute, then the same Quantity must also pass in the same time, thro' the Lungs, since the left Auricle and Ventricle are supplied from thence with Blood.

7. The Sum of the Surface of all the Vesicles of the Lungs of a Calf, were estimated *Vol. I. p. 242.* to be equal to 40000 square Inches, whence in proportion to their Weight, those of this Dog must be equal to 12121 square Inches: And as there were found in *Exper. VIII.* 4.34 Pounds or 113.684 cubic Inches of Blood to pass thro' the left Ventricle of that Dog, these cubic Inches divided by $\frac{1}{1620}$ part of an Inch, or 0.000672, the Diameter of the fine capillary Vessels, the Product is 169172 square Inches, which that Quantity of Blood would spread into. These divided by 12121, the Number of square Inches in the Vesicles of the Lungs give 13.95, so that they are $\frac{1}{13.95}$ th part of the expanse of the Blood: And allowing one half of these for the Space between the Cavities of the Blood-vessels, then the Sum of all the Cavities

of those Vessels, will be $\frac{1}{27.9}$ th part of the whole Expanse of the 4.34 Pounds of Blood, and consequently a Quantity of Blood equal to 27.9 times the Capacity of those Vessels must pass thro' them in a Minute. Whence we see by this Calculation, as well as by the above-mentioned small Proportion that the Lungs bear to the rest of the Body, that the Velocity of the Blood must needs be very much accelerated in them.

8. When we view in a strong Light the Blood circulating in the Lungs of a Frog, we see the Arteries as they pass on, sending Branches, which spread like a fine Net-work over the Surface of each Vesicle; and on some of these Vesicles we may very plainly see, the Blood when it has pass'd over little more than half their Surfaces, to enter corresponding capillary Veins, which thence unite in large Trunks; but on the greatest part of the Vesicles, the extream capillary Arteries, reach to the Verges of the Vesicles, and there enter at right Angles the Veins, which run along the Limits of those Vesicles; which Veins laying on the inner Sides of those Vesicles they are not visible like the Arteries: But when in here and there a Place I have clearly

ly

ly seen those Veins, I have then also seen the extream capillary Arteries, pour at right Angles their single Globules, into those much larger Veins; agreeable to what I saw in the injected Vessels in *Exper. XXI. Numb. 8.*

9. Now by this means the Blood has a much freer Passage thro' the Lungs, where it is requisite for it to move with much greater Velocity than in other parts of the Body. Whereas in some if not all the Muscles, by entering the finer capillary Vessels at right Angles its Velocity is much retarded. I have observed that where a long capillary Artery branches off at an acute Angle, that there the Velocity of the Blood is many times greater, than where it branches off at right Angles, which plainly shews the great Degree of Retardation which the Blood suffers by its passing off at right Angles. And this Retardation, which was necessary in order to prevent the Blood's passing too freely, must be very considerable, where it successively passes several times at right Angles, as in the Bowels, Urine and Gall-Bladders and other parts of the Body. On which account, as well as on account of the greater Length of the Arteries, a greater impelling Force was necessary

to drive the Blood thro' the great Artery, and its several Branches than thro' the Lungs; and accordingly the left Ventricle of the Heart is made much stronger, thereby to impel the Blood with a greater Force than the right Ventricle does.

10. On comparing the different Velocities of the Blood in the Muscles, and in the Lungs of a Frog; I found that the Blood moves in the parallel cylindrical capillary Arteries in the strait Muscles of its Abdomen at the rate of $\frac{1}{10}$ th of an Inch in nine Seconds of time, that is at the rate of an Inch in ninety Seconds, or one Minute and a half.

11. But the Blood flowed in the capillary converging Arteries of the Lungs with a much greater Velocity, viz. $\frac{1}{10}$ th of an Inch in the Time of eight beats of a Watch, which beat 16000 times in an Hour, that is in $\frac{1}{4.795}$ th Part of a Second. And there being 345.42 beats in nine Seconds, these eight beats of the Watch are $\frac{1}{4.317}$ th part of the abovemention'd nine Seconds; so that the Velocity of the Blood in the Frog's Lungs was forty three times greater than in its Muscles.

12. And I observed that the Motion of it thro' the Lungs was so free, that I could not
only

only see it sensibly accelerated at each *Systole* in the finest capillary Arteries, but also in their corresponding capillary Veins, tho' not in their larger Trunks.

13. And as a Frog's Heart has but one Auricle and Ventricle, the Blood is thrown by the same Ventricle, at the same instant, both into the Lungs and all over the Body; then since its Velocity is in Arteries of equal Diameters, forty three times greater in the Lungs than in the Muscles, notwithstanding it is impelled, by one common Impetus; this evidently shows, that it must have proportionably freer Passages through the Lungs; and consequently since it has a freer Passage, the Heat which it there acquires, by Friction against the Coats of the Vessels, will not be increased in proportion to its greater Velocity there, than in other Parts of the Body, but in some other intermediate proportion; for as the Blood meets with greater Resistance in passing from the Arteries to the Veins in other Parts of the Body, so were its Velocity equal in all Parts of the Body, it would acquire the greatest Heat, where it met with the greatest Resistance and Friction, which would not in that case be in the Lungs: But

as the Velocity of the Blood in the Lungs, is found to be much greater than elsewhere, so doubtless it there acquires its greatest Heat; yet in other Parts it acquires some, more or less in proportion to its different Degrees of Velocity and Friction.

14. I observed above *Numb. 8.* that tho' on some Vesicles of the Lungs, each extream capillary Artery had a corresponding capillary Vein, into which the Blood passes; yet I found that many of the extream capillary Arteries of other Vesicles, emptied themselves at right Angles, thro' the Sides of much larger Veins; the same I also found by Injections in *Exper. XXI. Numb. 8.* Whence, we may conclude, that though Anatomists have justly observ'd, that the Number of Veins in several Parts of the Body, are near double to the Number of Arteries, yet this cannot hold true, when we compare the Numbers of the extream capillary Arteries and Veins; for those of the Arteries must needs for the above-mentioned Reasons, far exceed those of the Veins in Number.

15. I shall hence take occasion to attempt an Estimate, tho' a very inaccurate one, of the Number of extream capillary Arteries in

a Man's Body, in the following manner, *viz.* Supposing as is mentioned in *Exper. VIII. Numb. 8.* that the Area of the transverse Section of the *Aorta* in Man, is 0.4187 Inch, and that the Length of the Cylinder of Blood, which is thrown out at each *Systole* of the left Ventricle, is 3.96 Inches; and the Area of a transverse Section of a fine extream capillary Artery being there set at 0.0000298 Inch: Then since equal Cylinders are as their Bases and Heights, the fine capillary Cylinder, equal to the Cylinder thrown out in each *Systole* of the Heart, will be 55639.98 Inches long: This Number multiplied by ten gives the Sum of that Column in $\frac{1}{10}$ Inches, *viz.* 55639.98 each of which was the Space the Blood flowed in nine Seconds. But this Length being to pass the extream capillary Arteries of a Man, in $\frac{1}{7}$ th part of a Minute, that is in $\frac{1}{8.88}$ of nine Seconds, (the Time in which the Blood moved $\frac{1}{10}$ th Inch in the Frog) tho' not with greater Velocity than in the Frog; therefore the Number of those extream Arteries in Man, must be proportionably increased by multiplying 55639.8 by 8.88, the Product is 494083, the astonishing Number of extream capillary Arteries. And if ac-

according to Dr. *Harvey* and Dr. *Lower*, double that Quantity of Blood be thrown out at each *Systole*, then the Number of extream capillary Arteries will also be double that Number, *viz.* 988166. And if the Velocity in the Lungs be 27.9 times greater as was found in *Exper. IX. Numb. 6.* then the Number of extream capillary Arteries there will be 3541713.

16. And how vastly more numerous are the many Branchings, Windings and Turnings of the Arteries and Veins, how innumerable the lymphatic Vessels, and secretory Ducts? and these all adjusted and ranged, in the most exact Symetry and Order, to serve the several Purposes of the animal Oeconomy; *So curiously are we wrought, so fearfully and wonderfully are we made!*

EXPERIMENT XI.

1. **A**S to the Force with which the Blood is impelled from the right Ventricle of the Heart into the pulmonary Artery, it seems impracticable to attempt the finding of it, by fixing a Tube to that Artery, in the same manner as to the carotid and crural Arteries

teries of living Animals, because the Animal must needs dye while it is doing.

2. The Area of the transverse Section of the pulmonary Artery, being in one part, before it divaricates into Branches, of the same Dimension with the Orifice of the *Aorta*, the Velocity of the Blood in that part, may be accounted the same as in the Orifice of the *Aorta*. But tho' the Quantities and Velocities of the Blood in passing out of both Ventricles, be the same, yet it does not thence follow, that their expulsive Forces must be both the same: For if the Blood in passing into the pulmonary Artery, finds less Resistance from the preceding Blood, than the Blood does in entering into the *Aorta*, then a less Force will expell it out of the right Ventricle with equal Velocity; and accordingly, as there is not so much Force required to drive the Blood thro' the Lungs, as thro' the rest of the whole Body, so we may observe, that the Substance of the Muscle of the right Ventricle, has not near the Thickness of that of the left. The following Experiments and Observations may give us some Light into this Matter, *viz.*

3. I fixed a glass Tube to the pulmonary Artery of a Calf's Lungs, and then thro' a
Tunnel

Tunnel poured warm Water into it ; then with a large Pair of Bellows fixed to the Wind-pipe, I alternately dilated the Lungs, to try if by that means, the Water would pass into the pulmonary Vein : But I soon found myself disappointed, for the Water flowed so freely, from the capillary Arteries thro' the Tunicles of the Vesicles, into the Vesicles themselves, and thence into the *Bronchiæ*, as to flow plentifully thro' the Windpipe, when it hung down in a depending Posture. At first I suspected that the Force of the Water, which was four Feet high, in the Tube affixed to the Artery, might have burst the thin Blood-vessels ; but I found it the same in several Try-als, on the fresh warm Lungs of Sheep, Oxen and Calves, even when the perpendicular Height of the Water of the Tube was less than a Foot ; and doubtless the Force with which the Blood is thrown into the Lungs, by the right Ventricle of the Heart, is greater than this.

4. And that so small a Force of Water could not burst the Blood-vessels, I was assured by the following Experiment, *viz.* I dissolved four Ounces of Nitre in a Pint of hot Water, into which Water there flowed from
the

the Cut-throat of a Calf, a Quart and a quarter of a Pint of Blood, which was kept in a diluted State by the nitrated Water. Having then fixed a Tube which was two Feet long to the pulmonary Artery of the above-mentioned Calf's Lungs, I poured gradually into the Tube, of the nitrated Blood, as much as the Artery and its Ramifications would contain, which was near a Quart, none passing that I could perceive into the pulmonary Vein. The Lungs were much dilated and looked very red : But notwithstanding the perpendicular Height of the Blood in the Tube was two Feet, yet no Blood passed thro' the Tunicles of the Vesicles, into the Vesicles and *Bronchiæ* ; for when the Windpipe was held downwards, nothing flowed out but a white Froth ; a plain Proof that when the Water was less than a Foot perpendicular in Height, in the foregoing Experiments, it did not forcibly break thro' the Blood-vessels, but must pass thro' Pores which were too fine, for the Globules of nitrated Blood to pass : Those Pores being perhaps something larger in the Lungs of a dead Animal, than when alive ; for upon Death all the Fibres of the Body are relaxed. When I cut a Slash into the Substance
of

of the Lungs, the nitrated Blood freely flowed out.

5. And that the capillary Arteries were not burst by the Force of the Water, seems farther probable from hence, *viz.* I fixed a Tube five Feet long to the pulmonary Vein of a Hog's Lungs, and poured in warm Water, which neither flowed into the pulmonary Arteries, nor among the *Bronchiæ*, an Argument that this Force did not burst the Veins, which some Anatomists say, have no Valves in them.

6. When I fixed the same Tube to the Windpipe of those Lungs, and poured in Water, it passed thro' the *Bronchiæ* and ran out of the Orifice of the pulmonary Artery, but not above one fifth so fast, as when its Course was the reverse, *viz.* from the pulmonary Artery to the *Bronchiæ*, in which case it run at the rate of a Pint in a Minute. Yet when Air was blown into the Cavity of the Lungs, thro' the Windpipe, none passed thence, either into the pulmonary Artery or Vein.

7. Another time I try'd also whether the thin Serum of a Hog's Blood would pass from the pulmonary Arteries, thro' the corresponding Veins of the Lungs, of the same Hog,
which

which Lungs were kept warm in Water ; the Serum passed most freely thro' into the *Bronchiæ*, but not into the Veins.

E X P E R I M E N T XII.

1. **I** Made an Incision of two Inches length between the Ribs into the *Thorax* of a Dog, on the right Side : At first opening the Lungs were dilated so as to fill up the Cavity of the *Thorax*, for they pressed against the inward Orifice of the Wound, and continued in this State for some time : But then as this right Lobe of the Lungs fell gradually more and more, so the Dog shewed more and more Uneasiness in breathing ; and on dilating or contracting the *Thorax* by the Action of the *Diaphragm*, the Air rushed briskly in and out at the Incision : But when the Orifice was covered by drawing the Skin over it, the Dog immediately breathed in a natural easy manner.

2. Hence we may observe, that the Lungs Continuance in a dilated State, for some time after the Orifice was made, must be owing to the Force of the Blood in the pulmonary Artery, in the same manner as the nitrated Blood in *Exper. XI. Numb. 4.* dilated those Lungs.

For

For since the Substance of the Lungs is of a very flaccid Nature, they must needs collapse, when the Air equally pressed on them both within and without.

3. Hence also we see that this Dilatation of the Lungs, which arises only from the Force of the Blood in the pulmonary Artery, is not alone sufficient to promote the Passage of the Blood thro' them ; but there is also requisite a farther Dilatation of the Coats of the Vesicles with inspired Air ; thereby probably to unfold the corrugated Extremities of the Arteries and Veins, and so to give the Blood a freer Course thro' them. For tho' in Experiment the first, it was observed that on deep sighing, that is on much dilating the Lungs, the Force of the Blood in the Horse's Arteries was greatly increased, which was occasioned by the Blood's flowing more plentifully thro' them when they are dilated than when fallen ; yet we cannot hence infer that the Blood flows more freely thro' them also when they are thus dilated by the Force of the arterial Blood, without the joint Dilatation also by the inspired Air.

4. But when for want of this Dilatation of the Vesicles by the Air, the Blood's free Course
was

was in this Dog thus retarded, it must needs therefore flow in much less Quantity to the left Ventricle of the Heart, which being thus deprived of a sufficient Supply, the Force of the whole arterial and venal Blood of his Body must be proportionably rebated, and consequently a less Supply coming from the Veins, to the right Ventricle of his Heart, the Force of the Blood in the pulmonary Artery, being much abated, so as not to be sufficient to dilate the Lungs, they collapse : So that if a Tube had at the same time been fixed to one of the carotid Arteries of this Dog, no doubt but the Blood would have subsided considerably in the Tube.

5. But when on straining, the Dog, by the joint Action of all the Muscles of his *Abdomen*, thereby compressed the venal Blood forcibly up into the ascending *Vena Cava*, the right Ventricle of the Heart being thereby more plentifully supplied with Blood, impelled it also more forcibly into the pulmonary Artery, so as to make the collapsed right Lobe of the Lungs instantly to dilate, so vigorously to push the lower part of the Lobe, one, two, and sometimes three Inches length out thro' the Incision ; and that after he had lost half a
Pint

Pint of Blood. But when more than half of his Blood was evacuated, then the right Lobe did no more dilate on his straining.

6. Hence there does not seem to be so much danger as has been imagined in the *Paracentesis* or Incision into the Thorax, in order to take away an Abscess, &c. for tho' while the Orifice was open, this Dog breathed with Difficulty, yet the left Cavity of the Thorax being by means of the *Mediastinum* still closed, the left Lobe of the Lungs play'd too and fro to such a Degree, that the Dog breathed enough to keep up the Circulation of the Blood for more than a Quarter of an Hour, that I purposely try'd: And the Uneasiness in breathing, not increasing in so long a time, it is reasonable to believe that he would have lived in that manner, for some Hours. But if the left Cavity of the Thorax had also been open at the same time, he would doubtless have died soon. Now supposing one Cavity of a Man's Thorax to have been open'd, and the Operation that was thereby intended, perform'd; if just before the Incision be closed up, the Man be made to strain, and thereby strongly contract all the Muscles of his *Abdomen*, the fallen Lobe of the Lungs

will

will then immediately dilate, upon which the Orifice being instantly covered with a Plaster, the Man will then breathe as free as ever.

Qu. Will strongly girding or compressing the Abdomen do the same thing?

7. But in case the Lungs themselves are pierced with a Sword or Bullet, then this straining must needs be hurtful, because it will much increase the Effusion of Blood.

8. Hence we may see how dangerous it is for those, whose Lungs are so weak as to be subject to break, to use strong Exercise or Motion; for when a Man either strains, or is using any violent Exercise, so as either to have the Blood impelled more forcibly into the right Ventricle, or by running, &c. to have that Ventricle beat instead of seventy five times, one hundred and twenty times in a Minute, then the Blood must needs be very forcibly impelled into the Lungs.

9. In which case the Blood being greatly accumulated in the pulmonary Artery, the Lungs will consequently be considerably dilated, so as to subside but little in Expiration; which may be the Cause of those small but quick Inspirations and Expirations, which Men make in strong or quick Motions. This is

what those whose Lungs are considerably wasted or otherwise defective are but too sensible of, even in a small Degree of Motion; for in this case, the natural Course of the Blood thro' the defective Lungs, being much retarded, there is immediately a great Accumulation of Blood in the pulmonary Artery, occasioned by the accelerated Pulses of the Heart: The Capacity of whose right Ventricle, is proportioned to a sound State of the Lungs, as all the Parts of an Animal, both solid and fluid, are in a healthy State, admirably tempered and adapted to each other; but in a defective State of the Lungs, they are too easily surcharged with Blood, so that the unhappy Person is ready to be choaked; that is the Blood does with very great Difficulty and in but small Quantities pass the Lungs, to supply the Demands of the left Ventricle of the Heart, without which Life instantly ceases.

10. There is probably also the like Accumulation of Blood in the pulmonary Artery, in pleuretic Cases, when the Blood by its Sickness does with difficulty pass, tho' impell'd with such Force as to distend the Vessels, and thereby cause pungent Stitches. One reason why the Effects of a fizy Blood are usually

sooner felt in the Lungs or *Pleura*, than in other parts of the Body, may be this, *viz.* by *Exper. CXIII. Vol. 1st, p. 253.* it was found that when a Gage was fixed to the Thorax of a live Dog, the Spirit of Wine rose in the Gage, about six Inches in ordinary Inspirations, and twenty or thirty Inches in laborious Inspirations; whence it is evident that there is then so much less Pressure of Air in the *Thorax*, both on the *Pleura* and Surface of the Lungs, the Consequence of which is, that more Blood will at that time flow into the Vessels, as they are then less compressed by Air, as is the known Case in Cupping; and which is further evident from the following Experiment, *viz.* I slit Holes thro' the intercostal Muscles on each side of the *Thorax* of a Kitten, and then placing it under an Air Pump Receiver, it was killed in *Vacuo*; when on opening the *Thorax* I found the Lungs very red with coagulated Blood, which had flowed more freely into its Blood-vessels, on the Weight of the Atmosphere being taken off of its Vesicles, both within their Cavities, and on the Sides next the *Thorax*: Whereas if a Kitten be killed in *Vacuo*, without having Orifices made into its *Thorax*,

the Lungs will in this case be very white; for while the Air is drawn away from the Vesicles thro' the *Bronchia*, the Air which is included in the Cavity of the *Thorax*, being dilated compresses the Lungs, and also the Blood out of its Vessels, on which account they are in this case found to be white. And that the Air in the *Thorax*, thus compresses the Lungs in *Vacuo*, is evident hence, *viz.* If the Body of a Kitten, as soon as killed, be cut asunder, just below the Diaphragm, and a Weight be then tyed to the Head, so as to make that obtruncated Part sink under Water, if in this Position it be placed in *Vacuo*, the Diaphragm will dilate considerably and subside again, as soon as Air is let into the Receiver, and it will be the same if not immersed in Water: A plain Proof, that there is Air in the Cavity of the *Thorax*, which while it thus, by its Expansion, dilates the Diaphragm, must needs at the same time compress the Lungs, in which State they are found on opening the *Thorax*: Whereas, Lungs taken out of the *Thorax* do dilate in *Vacuo*, and continue so when Air is let into the Receiver. Another Reason why the ill Effects of such sily Blood, do probably fall

sooner on the Lungs than on other Parts, is, because much greater Quantities of Blood, do, in proportion to their Bulk, pass in equal Times thro' the Lungs, than thro' any other Part of the Body, and consequently a too viscid State of it, will soonest be felt there : Or in the *Pleura*, where Anatomists observe, that the Blood has also a very free and short Circuit, by the intercostal Arteries, and thence thro' the *Azygos* Vein to the Heart : On which Account there being also a very plentiful Flow of Blood into the *Pleura*, the Effects of its Sizeness are frequently first felt there ; and that oftner on the left than the right Side : Probably because the *Aorta* laying on the left Side, the Blood is thence impelled with greater Force, into the shorter left, than into the longer right intercostal Arteries. Which Inconvenience, as is well known, is often greatly rebated by decreasing the Quantity of Blood in Bleeding. And thus also in plethorick, asthmatic and other Cases, the Lungs are often immediately very sensibly relieved by abating the Quantity of Blood.

II. The strong cordlike Beatings of a pleuritic Pulse, has by some been attributed to the crisping of the Fibres of the Coats of the Ar-

teries by the pleuretic Heat ; but it seems to be more probably owing to the Siziness of the Blood, which on that Account passing with greater Difficulty the capillary Arteries, it must consequently be more accumulated there, by the impelling Force of the Heart. So that if a glass Tube were fixed to the Arteries of a pleuretic Animal, it would probably mount much higher than in a healthy State of the Animal, especially in the beginning of the Distemper, before the vital Strength be impaired.

12. Those whose Business it is to speak long aloud in publick, do very sensibly find the Difference before and after Dinner, in which case it is much more laborious to speak, the Lungs being incommoded with a greater Quantity of Blood then accumulated in the pulmonary Artery.

13. And as it was before observed that the Dilatation of the Lungs, greatly promotes the free Passage of the Blood thro' them, and thereby the more invigorates and exhilarates ; so we may observe that those who in speaking in publick, habituate themselves to speak with well dilated Lungs, do it with much more Ease to themselves, and greater Satisfaction to
the

the Hearers, than those who draw in but little Breath; who are thereby not only incapacitated from speaking loud, but are also at the same time greatly incommoded and fatigued for want of a free Passage of the Blood through the but little dilated Lungs. This is indeed unavoidable in some who labour under the Inconvenience of a narrow *Thorax*; neither are such Persons usually so healthy, as those who have a large *Thorax* for their Lungs to dilate in, which is commonly with good Reason looked on as a prime Symptom of a robust and healthy Constitution.

14. But the healthy Dilatation of the Lungs, and what is consequent thereon, the free Passage of the Blood thro' them, is much oftner incommoded by Intemperance and Excesses in eating and drinking, than by any Defect in the Formation of the *Thorax*. For these Excesses incommode the Lungs not only by surcharging them, with too great Quantities of inflamed Blood, but also hinder the free Dilatation of them, by filling the Stomach and Bowels so full, as thereby not only greatly to hinder the Action of the Midriff in descending, and thereby dilating the *Thorax* and Lungs; but also retard the Motion of the

Blood in the Coats of the much distended Bowels : Hence habitual Excesses, as they usually occasion many kinds of Disorders in other Parts of the Body, so do they very often distemper the Lungs.

15. And here it may not be improper to observe, as it naturally occurs from the foregoing Considerations, the great Benefit of Exercise even to the temperate Liver ; for it not only by the meer Effect of Motion agitates the Blood in all Parts, but also gives it a brisk Circulation, not only by its increasing the Number of the Systoles of the Heart, but also by giving it a freer Course thro' the more dilated and agitated Lungs : Which Dilatation Exercise also makes more free and easy, by promoting Digestion and the Descent and Evacuation of the Contents of the Bowels, whereby not only the Midriff can more freely act and dilate the *Thorax* and Lungs, but the Blood also can have a freer Passage thro' the Coats of the Stomach and Guts. Thus in whatever View we consider the Animal Oeconomy, many cogent Arguments for Temperance and Exercise do always occur.

16. When we see in *Exper. XI. Numb. 6.* how freely the Serum passed from the pulmona-

ty Artery into the Cavity of the Vesicles and *Bronchiæ*, 'tis no wonder that so great Discharges of Humours are often made thro' the same Passages, when the Blood is much diluted, by being in Colds surcharged with too much obstructed perspirable Matter, or otherwise disordered : Hence also some Asthma's have their Origin.

17. " Sir *John Floyer* in his Treatise of the " Asthma assigns the immediate Cause of it " to be the Straitness, Compression, or Con- " striction of the *Bronchia*. He observes that " the Fit of the Asthma happens suddenly, " thro' the Effervescence of the Blood, occa- " sioned by external Causes which separate the " *Lympha lactea* from the Blood, which stops " in the swelled Glands of the Lungs." This Notion of his seems to be confirmed by what is found to be constantly the Case, when Water instead of Blood flows into the Arteries of a Dog, in which case, as is observed in *Experiment XIV. Numb. 5.* all the Muscles of the Dogs were convulsed : And the like Defluxion of thin serous Humours on the Nerves and muscular Fibres of the *Bronchiæ* and Vesicles may probably by contracting them in the same manner occasion the abovementioned Straitness and Constriction of the *Bron-*
chia

chiæ in the Fit of the Asthma. This De-
fluxion of Serum, he says, evidently appears
in loose Stools, Fluxes of Urine, great Spit-
ting, Drowsiness of the Head, in the begin-
ing of the Asthma Fit.

EXPERIMENT XIII.

I. **W**E see in *Experiment X.* with how
much greater Rapidity the Blood
passes thro' the Lungs, than thro' any other ca-
pillary Vessels of the Body; whence we may
with good reason conclude, that it principally
acquires its warmth, by the brisk Agitation it
there undergoes: This we find by daily Ex-
perience, that an accelerated Motion of the
Blood by Labour or Exercise, does constantly
increase its Heat; whence we may well infer,
that it acquires its Warmth chiefly in the Lungs,
where it moves with much greater Rapidity,
than in any other capillary Vessels of the Body.
And that the heat of the Blood arises princi-
pally from this Friction, is hence also pro-
bable, that on strong or brisk Motion of the
Body, its Heat increases much faster, than it
could by any effervescent or fermentative Mo-
tion. And *e contra*, on a Cessation of the
Blood's

Blood's Motion, either when extravasated, or on Death, the Blood then cools as fast as any other warm Fluid of equal Density, which has no Effervescence in it.

2. As fermenting and effervescent Mixtures acquire Heat by the brisk Agitation and rubbing of the Particles of the effervescent Mixture against each other; so the Globules of the Blood may well acquire their Warmth, by being briskly agitated, in passing with great Velocity through innumerable divaricating and converging fine Canals.

3. And *Qu.* Is not this a principal use of the red Globules, which are the most compact and firm part of the Blood, and withall elastick, whereby they are the more susceptible of Warmth from this Agitation of Friction? Their red Colour intimates their abounding with Sulphur, which makes them the more susceptible and retentive of Heat, than those Bodies which have less of it in them: For the more watery a Body is, the less susceptible it is of Heat; whence 'tis with good reason concluded, that if pure Water were to pass through the Blood Vessels, with a Velocity equal to that of the Blood, yet it would thereby acquire no Heat. This is what

we have many instances of in effervescent Mixtures, several of which will, with seemingly equal Degrees of Effervescence, acquire very different Degrees of Heat, which may depend either on the different Nature or Texture of their component Particles, or the different manner of the Action of those Particles on each other: Thus even of solid Bodies rubbed against each other some will sooner acquire a burning Heat than others. *Leeuwenhoeck* has observed that the Blood of Fishes, which is much cooler than that of other Animals, has a greater Proportion of *Serum* in it: The Blood of a Land Animal has twenty five times more Globules in it than that of a Crab. If according to Dr. *Jurin's* Estimate in *Mott's* Abridgment of *Philos. Transact. Part II. p. 143.* the Quantity of the Blood Globules be $\frac{1}{4}$ th of the Blood: And if as he has estimated it a Diameter of a Blood Globule be $\frac{1}{3240}$ of an Inch; then the $\frac{1}{4}$ th of the Cube of 3240, viz. 8,503,056,000 will be nearly the Number of red Globules in a cubic Inch of Blood: And the Distance of the Centres of the Globules from each other will be $\frac{4}{3240}$ Inch.

2. Dr. *Boerhaave* observes that Oil is capable of a much greater Degree of Heat than Water; and that, like the Blood Globules, abounds with Sulphur, which is found strongly to attract Light and Air, which are both very active Principles.

5. Now many solid Bodies, which having acquired Warmth by rubbing, being found to be electrical, it put me upon trying whether any, much agitated Fluids, were so too.

6. Having therefore put half an Ounce of Mercury into a two Ounce Vial, I shook it briskly to and fro for a considerable time, and then laying the Vial down Sideways on a Table, I rolled it very gently, so as to make the Edge of the Mass of Mercury approach gradually to innumerable small separate Particles of Mercury which adhered to the Sides of the Vial; where, with Pleasure I could see some of those Particles attracted to, and others repelled from the Mass of Mercury; which plainly shewed the electrical Quality it had acquired by Agitation: Yet Mercury heated by an Effervescence with double *Aqua fortis* had no Electricity.

7. I poured into a thin *Florence* Flask two Ounces of cold Water, and to that a sufficient

cient Quantity of Oil of Vitrol, which made it as hot as my Hand could well bear. Then I approached the bottom of the Flask to some Tinsel, Down and Hairs, but it neither attracted nor repelled any of them. And it was the same when a strong Effervescence was made with double *Aqua-fortis* and Filings of Iron.

8. It was observable that these sudden hot Effervescences which arose in the Mixture did not sensibly affect Tinsel, &c. tho' placed near the bottom of the Flask before the Mixture was put in ; a probable Argument that no subtile Matter rushed suddenly thro' the Pores of the Glass to cause that Effervescence. Whereas electrical Effluvia do most easily pass thro' the Pores of a glass Sphere which has been rubbed so as to acquire Electricity.

9. Some Tinsel, &c. being laid on the Outside near the Bottom of a Flask, into which some Blood had just flowed from a Hog, they were not attracted. I placed the Tinsel thus on the Outside of the Glass, thereby to intercept the warm Vapour of the Blood which I apprehended might hinder the Attraction, if the Tinsel had been approached to the Surface of the Blood.

10. I put two Ounces of the same Blood into a Glass full of Cells or Cavities made purposely to incorporate Oil and Vinegar: stopping the Mouth of the Glass close, I tied it to a Pole which was ten Feet long, the other end of the Pole being firmly fixed: The Glass thus fixed was carried to and fro with long and swift Vibrations of the Pole, for some Minutes; but the thus agitated Blood which was of a very florid Colour would not attract Tinsel, &c. either thro' the Glass or when poured out on a Plate.

11. Whereas Blood thus agitated has no Electricity, tho' shaken Mercury has, may not this be owing to the watry Part, which abounding much in Blood, as also in the above-mentioned effervescent Mixtures, does probably check Electricity, tho' not the Heat which is acquired by the mutual rubbing of the effervescent Particles against each other? Electrical Experiments are found to succeed best in a dry Air; thus if a glass Tube be rubbed to such a Degree as to be strongly electrical, it will instantly lose that Electricity, if moisten'd with Water either cold or as warm as the Tube: So that we cannot from this Want of Electricity in the Blood, conclude that its Heat
is

is not acquired by brisk Agitation and Motion in the Blood-vessels.

12. But we have in the *Muscle-shell* Fish a remarkable Instance of the Electricity of its Blood Globules : For if a Piece of one of the *Bronchiæ* or Gills be cut off and put into a small concave Glass with three or four Drops of its Liquor, and be then placed under a double Microscope, the Blood may be seen greatly agitated in the fine Vessels ; and at the cut Edge of the Piece of Gill may with great Pleasure be seen, many Blood Globules repelled from the cut Orifices of the Blood-vessels, and attracted by other adjoining Vessels ; also other Globules rolling round their Center, and repelling each other ; whence it is plain that Bodies, by brisk rubbing and twirling about may acquire, in a watry Fluid, both attractive and repulsive Vertue or Electricity. If fresh extravasated Blood be placed before a Microscope, the Globules may be seen by mutual Attraction to aggregate and form greater Globules.

13. But tho' it may be doubtful whether the Blood Globules, by reason of the warm watry Fluid in which they float, do acquire an electrical Vertue or no, in passing with great Rapidity and strong Friction thro' the innumerable

nable capillary Vessels of the Body, especially those of the Lungs: Yet as electrical Bodies acquire greater Degrees of Electricity when rubbed in a cold than in a hot Air, so it is reasonable to think that the Globules may acquire considerable Degrees of elastic Vibrations in passing the Lungs: For while by the extraordinary Frictions they there undergo, they are much heated and dilated, they are at the same time refrigerated and contracted by the fresh Air that is continually taken into the Lungs: Where, by reason of the great Expansion of the Surfaces of all the Vesicles of the Lungs, a very large Surface of the Blood is expos'd to an equally large Surface of Air contained in those Vesicles, whose Coats are so extreamly thin, that those two Fluids are supposed to be $\frac{1}{1000}$ th part of an Inch within Contact of each other, so that like blended Liquors, they must needs have a considerable Effect on one another, the Air in cooling the Blood, and the Blood in warming the Air.

14. Which Effect of the Blood on the Air in the Lungs is so considerable, that tho' that Air is by Inspirations blended with a good Quantity of fresh cool Air, no less than 1200 times in an Hour; yet by holding the Ball

of my Sp. v. Thermometer in my Mouth for a considerable time, while I breathed warm Air on it, but drew in the cool Air in Inspiration thro' my Nostrils, the Sp. v. rose in the Thermometer from ten Degrees, the Heat of the external Air, to forty six Degrees above the freezing Point ; so that in the $\frac{1}{120}$ th Part of an Hour, or 3" of a Minute the inspired Air had acquired thirty six Degrees of Heat. The natural State of the Blood, in which State it then was, being sixty four Degrees, and that of the external Air ten Deg. it was therefore fifty four Degrees colder than the Blood, yet in so short a time it acquired thirty six Deg. of Heat.

15. The Quantity of Blood, which passes thro' the Lungs of a Man in a Minute, being estimated in *Experiment VIII. Numb. 12.* to be 8.74 Pounds or 228.8 cubic Inches ; and the Quantity of Air drawn in at each Inspiration being forty cubic Inches, as estimated in Vol. I. p. 243, they will amount to eight hundred cubic Inches in the twenty Inspirations of a Minute ; so that this Quantity of Air will be to that of Blood as 3.48 is to 1 : The Blood's specific Gravity to that of Air is as 841 to 1.

16. I communicated these *Data* to Dr. *Desaguliers*, who together with Mr. *Ch. de Labely*, who was then present, both agreed in the following Calculation of the Degree of Refrigeration which the Blood received from the inspired Air, *viz.*

17. Actual, is to sensible Heat, as felt by the Hand or shewn by a Thermometer, as Momentum is to Velocity.

18. Sensible Heat multiplied by the Quantity of Matter, gives actual Heat or Momentum of Heat.

19. Therefore actual Heat divided by Matter gives sensible Heat, as Momentum divided by Matter gives Velocity.

20. Therefore as we increase the Matter, we decrease the sensible Heat.

21. What gives sixty four Degrees of sensible Heat to one, gives but one Degree of sensible Heat to sixty four.

22. The specific Gravity of Blood to that of Air, being as 841 to 1, if a Bulk of Air, which is to a Bulk of Blood, as 3.48 to 1, be so condensed as to be reduced to the Bulk 1, or the same Bulk as the Blood, it must be brought to a specific Gravity so much greater, than the Disproportion or specific Gravity of

Blood to Air will be 241.6 to 1, because $\frac{841}{3.48} = 241.6$.

23. The Question is reduced to this, *viz.* what gives thirty six Degrees of sensible Heat to $\frac{1}{241.6}$, how much will it give to one?

24. The Answer is $36 \times \frac{1}{241.6} = \frac{36}{241.6} = 0.149$ or about 149th of a Degree.

25. Now as $3'' 0.149 :: 60 :: 2.98$, therefore in one Minute the Heat added to the Blood in the Lungs will be 2.98 Degrees, the whole Heat of the Blood there being $64 + 2.98$ Degrees = 66.98.

26. So that if a Man holds his Breath one Minute, the Blood in the Lungs from sixty four Degrees of Heat will be increased to 66.98, and in two Minutes (during which time several Men hold their Breath or can blow without ceasing, as *Grano* the Trumpeter can) the Heat will be got up to 69.96.

27. But then upon the Mixture of that warmer Blood with the rest of the Mass of Blood, the sensible Heat must be diminished; for that which in two Minutes, gives to the Quantity of Blood in the Lungs the sensible Heat of 5.96, will give as much less sensible Heat to the whole Mass of Blood, as the Quantity of the whole Mass is greater than one, which

which was esteemed the Quantity of Blood in the Lungs.

28. Calling therefore the whole Quantity of Blood x , or the Proportion of the whole Quantity of Blood in the Lungs, Ratio $x : 1$; $x : 1 :: 5 : 96$ (Degrees of sensible Heat in the Lungs acquired in two Minutes) to the Degrees of sensible Heat of all the Mass of Blood acquired in the same time, that is $\frac{5.96}{x}$. This Quantity must be added to the Heat of the whole Blood for every two Minutes, if the Breathing be stopped so long.

29. Now the whole Quantity of Blood in a Man being estimated twenty five Pounds: The Quantity of Air forty cubic Inches = 1.24 Pounds, that is, supposing its specific Gravity to be equal to that of the Blood ; but the Bulk of the Blood has been found above to bear a Proportion to the Bulk of Air in the Lungs as 1 to 3.48, therefore if we say as 3.48 to 1. so the Weight 1.24 Pounds (of the Bulk of Blood equal to that of Air in the Lungs) is to the Weight of the real Quantity of the Blood in the Lungs = $\frac{1.24}{3.48} = 0.356$ of a Pound ; consequently multiplying the Heat acquired in two Minutes 5.96 Degrees into the Quantity of Blood, it is in 0.356 of a

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Pound

Pound we have the whole additional actual Heat $= 5.96 \times 0.356 = 2.12176$, which being divided by the whole Mass or Quantity of Blood $= 25$ Pounds gives $\frac{2.12176}{25} = 0.08487$ of a Degree. So that the whole Mass of Blood, by avoiding Inspiration for two Minutes, will be increased in Heat from sixty four Degrees to 64.08487 Degrees.

30. And if the Heat of the Blood increases as the Times, then in half an Hour the whole Mass would be increased in Heat from sixty four Degrees to $64 + 1.27305 = 65.27305$ Degrees.

31. Dr. *Boerhaave* relates the remarkable ill Effects of breathing in a very hot Air. For having caused a Sparrow to be put into a Sugar Baker's drying Stove, the Heat of which was so great as to raise the Mercury in *Fahrenheit's* Thermometer to a hundred and forty six Degrees; that is 0.78 more than the natural Heat of the Blood, *viz.* ninety two; the Sparrow after about a Minute expressed great Uneasiness and dyed in seven Minutes. A Cat also put into the same Stove expressed great Uneasiness in a Minute and dyed in about sixteen Minutes, it was as wet with Sweat as if dipped in Water; but a Dog which was put in

in at the same time did not sweat; after seven Minutes he panted much for Breath, and in a Quarter of an Hour expressed very great Uneasiness, soon after which he grew faint and died in twenty eight Minutes: He drivelled all the time a great Quantity of red Foam, which did stink so intolerably, that a strong labouring Man who went near it, was almost struck down in an instant with this Stench.

32. He observes in this Experiment the dire Effects of this Degree of Heat, how soon it brought on a most acute Distemper, with very violent and mortal Symptoms: How suddenly the Humours were changed, from a healthy to a nauseous putrid State, more pestilential and deadly than the rottenest Carcase, how greatly the Humours must be altered in so short a time, to make the *Saliva* red. He also justly observes that these were not the mere Effects of the Heat of the Stove; for if the Flesh of a dead Animal had been hung up there, it would have dried, and not have turned to pestilential Corruption; which must therefore arise from the Friction caused by the vital Motion of the Blood in the Lungs; where it being in this case not at all refrigerated, did thereby acquire a greater Heat

than that of the Stove; whence its sudden Tendency to Putrefaction, the Oils, Salts and Spirits of the Dog being thoroughly putrified in twenty eight Minutes.

33. He observes also that when a Man breathes an Air as hot as his natural Heat, he soon finds such a Difficulty of Breathing, that he cannot long endure it, but earnestly pants after cooler Air, which invigorates, while hot Air weakens and dispirits; for neither Animals nor Plants can long bear a hot Air, without Intervals of cool refreshing Air.

34. Whence he justly infers, that as the Blood is most heated in the Lungs, by reason of the great Velocity and Friction it there undergoes, so it is there also most refrigerated. *Elementa Chæmiæ Tom. I. p. 275 to 278.*

35. And *Tom. II. p. 378*, he observes that the natural Heat of the Blood is not far from the coagulating Point, which is a hundred Degrees, and the natural Heat ninety two: Whence it may be concluded that a Fever Heat must tend to coagulate the Blood; in order to resist which Tendency, Nature is then under a Necessity of greatly accelerating its Motion thro' the circulatory Vessels, which,

which, as it promotes Attenuation, does also at the same time increase Heat.

36. As the natural Heat of the Blood is not far from the coagulating Degree, to which and a much greater Degree, it does we see soon rise, if not frequently refrigerated, by the Inspiration of fresh Air : So this makes it very probable that one considerable Use of the Lungs, is to refrigerate the Blood ; Attenuation and separating the Blood Globules is also doubtless another great use of them : For tho' the Blood Globules are separated and pass single, from the innumerable extream capillary Arteries of the rest of the Body, to the corresponding Veins, yet the venal Blood is not florid, which Floridness may in a good Measure be owing to the strong Agitation, Friction and Comminution which it undergoes in passing, with much greater Velocity thro' the Lungs than other parts of the Body ; in like manner as in *Exper. XIII. Numb. 10.* the Blood which was much agitated in a closed Glass Vessel, was observed to be very florid, not only on its Surface but thro' its whole Substance as arterial Blood is. 'Tis probable also that the Blood may in the Lungs receive some other important Influences from
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the Air, which is in such great Quantities inspired into them. It has long been the Subject of Enquiry of many, to find of what use it is in Respiration, which tho' it may in some Respects be known, yet it must be confessed that we are still much in the dark about it.

37. As the Air in ordinary Inspirations and Expirations, passes freely and easily too and fro with small Velocity, so it cannot have any considerable Effect on the Blood by any impulsive Force: Nor yet by any additional Gravity arising from the Form of the Lungs, which Gravity, upon a Calculation that the Sum of all the Areas of the Vesicles is equal to a hundred and fifty two square Feet, has by Dr. *James Keil* been estimated to be equal to 50443 Pounds Weight, there being a manifest Mistake in the Argument. For suppose a cubic Foot of any solid or fluid Matter, to be divided into an hundred *Lamina* or Plates, each of these Plates when spread abroad will be pressed with the whole Weight of the Atmosphere; and when laid on each other in the Form of a cubic Foot, they will then also each of them sustain the same Weight of the Atmosphere; besides that in this Position, all
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except the uppermost, will have the additional Weight of the Plates which lay on them; whence it appears, that the Blood will have less Weight on it, when spread into a broad thin Expanse, than when in a larger Mass.

38. As the Blood acquires different Degrees of Warmth according to the different Degrees of Velocity with which it is carried on, and also in proportion to the different Diameters, Laxity or Tenseness of the Vessels; hence in a more lax State of the Fibres of the Vessels, the Blood will be cooler, more viscid, and less florid and digested: But *vice versa* in a more firm and tense State of the Coats of the Blood-vessels the Warmth of the Blood will be the greater, it being in those more hale vigorous Constitutions, impelled more briskly thro' the tenser capillary Vessels; whence a constant greater Warmth and Vigour in these Constitutions, and consequently a more highly digested and attenuated Blood. But when the Heat rises to the Degree of a Fever, then it often putrifies the Blood.

39. Tho' we cannot on the one hand reasonably suppose, that there is a strong repelling Degree of Ferment or Effervescence in the Blood, in its healthy State, so neither on the
other

other hand can it be imagined to be a meer languid inert Fluid : For it must needs be that the Parts of a Fluid, which is stored with such active Principles, will be in a vibrating State, while actuated with so considerable Degrees of Friction and Heat, as the Blood is : Which Vibrations are restrained within due Bounds, by the attractive Power of the Sulphur, which abounds in the Blood to such a Degree, that tho' great Quantities of fermented Liquors are daily taken in, and mixed with the Blood, yet they are thereby so restrained, as not to be able to raise the Blood, into a strongly repelling Degree of Ferment ; tho' they much increase its Effervescence and Heat. When these Liquors are taken in to an intemperate Degree, they do then raise the Effervescence of the Blood, to such a feverish Heat, as requires many Hours time, before it be abated, and the Blood brought to a right Temper again.

40. When we consider that all vegetable Ferments are chiefly carried on by the Action and Reaction between the Air and sulphurous Particles ; and also that these Principles, with which the Blood is stored, do in a fixt State, form Tartar in the Urine ; and at the same
time

time remember, that it is observed by Physicians, as an Indication that the Fever abates; when the Urine deposits a redish Brick coloured Sediment, that is, a Tartar; have we not reason to suspect that this Tartar, while it was in an elastic State in the Blood, greatly contributed to its feverish Heat; which Heat therefore abates in proportion as these active Principles are subdued and reduced to a fixed State, fit to be carried off by Urine or other Evacuations.

41. The right healthy State of the Blood must consist in a due *Equilibrium* between these active Principles, so as not to have them too much depressed and fixed on the one hand, which might tend to an acid Acrimony, nor too much raised and exalted on the other hand, which makes it tend to an alkaline Acrimony. When we consider therefore by what an innumerable Combination of Causes this *Equilibrium* is liable to be disturbed, we cannot wonder that our Health is so often interrupted, and the Period of Life most uncertain as to its Duration. It is wonderful that the fixt organical Parts of our Bodies, which are of a most curious and delicate Texture, should hold it out so long, without being dis-

ordered

ordered or wearing out ; but the Wonder is greatly heightened, when we consider for how many Years Continuance, the nice healthy Ballance of Power, between the active Principles of the Blood is maintained, notwithstanding the many rude Shocks it meets with, either from unkindly Food, or inclement Seasons, and above all from Intemperance.

42. When the morbid Blood is so gross or viscid as not easily to pass the finer capillary Vessels, it being thereby greatly retarded in its Motion, causes the cold Fit which usually preceeds a Fever or Ague. And as warm Liquors, such as Urine, &c. are observed to turn from clear to turbid and to deposite Sediment as they cool, and when warmed again to resorb that Sediment and become clear ; so 'tis probable that as the Blood grows cool on the first coming on of the Fit, that Coolness may be greatly increased, by the then more thick turbid State of the Blood ; its Motion being thereby proportionably retarded : But when after some time the retarded Blood, which is at this time, for want of a free Passage, probably accumulated in the Arteries, so as to be at length forcibly impelled thro' the capillary Vessels, then by the greater Friction

tion of its gross Parts it acquires a burning Heat; which Heat is prolonged to different Periods of Time in proportion to the Quantity of the gross morbid Matter, till it is at length either sufficiently attenuated by repeated Circulations and Diluters, or else ends in Death.

43. If, as was before observed, the Blood grows more thick and turbid as it cools, it may then be owing to this, that bleeding and purging, which as they greatly cool may also thereby bring on an Ague Fit, which is well known to be the Consequence of those Evacuations, when the Blood is at any time in an aguish Disposition.

44. A too relaxed State of the capillary Vessels may in an aguish Constitution greatly contribute to the Return of the Fits, for the Blood acquiring by this means, in a determinate time, too great a Viscidity, the next periodical Fit may thereby be brought on.

45. The gross tartarine gouty Particles are aptest to settle and cause inflammatory Obstructions in the Extremities of the Body as in the Feet and Hands, where the progressive Force of the Blood is least, as being farthest from the Heart. And when these Humours fix in the Trunck of the Body, they are apter

to settle in the Stomach than in the Guts ; because in the former they have as much greater a length of capillary Vessels to retard the Motion of the Blood, as half the Circumference of the Stomach is greater than that of the Guts ; for the Arteries of the Stomach do not enter its Coats on one side only, as those of the Guts do, but it is supplied with Blood by Arteries which enter, some on the upper and some on its lower side ; their converging Branches inosculating about the middle of the Sides : Without this necessary Precaution, the Motion of the Blood must needs have been there greatly retarded, if it had entered its Coats only at the top or bottom ; because in that case it must have passed thro' capillary Vessels of double the Length that they now are.

46. When any gross Matter from an Ulcer returns into the Course of Circulation, it first causes a Shivering by its Obstructions ; but when this Matter is impelled by the Force of the circulating Blood thro' the fine capillary Vessels, it then frequently raises a feverish Heat by its greater Friction in those Vessels.

47. In

47. In dropfical Cafes when the Blood is poor and watry the Patient complains of great Degrees of Cold, the Blood being very defective in a fufficient Quantity of red Globules to give it Warmth. Which yet at Intervals will be raifed to a feverifh Heat, for want of a due Quantity of thin Serum, and by the Return of fome of the rancid extravafated Humour into the Courfe of Circulation.

48. Thus alfo when too much Blood has been evacuated, it is long before a Man recovers that Lofs, and he is too cool, not only for want of a fufficient Quantity of Blood to be impelled with Vigour thro' the capillary Veffels, where, as we have before feen, it meets with much Refiftance, but principally thro' too great a Deficiency of red Globules, whereby to procure it a fufficient Degree of Warmth, as alfo thereby to keep the Serum diluted by their innumerable runnings to and fro: For if Quantity of Fluid alone would make good the Defect, there would be a fufficient Quantity of Fluids in the Arteries and Veins, fome time after each Meal; but this alone will not compensate for the Lofs of Blood. Befides, when the Serum of the Blood is too thin, the Globules are much apter to coalefce, for the thin

ner the Fluid the more readily will mutually attracting Particles, which swim in it, coalesce. But on the contrary too great a Proportion of Globules disposes the Blood to be inflammatory.

EXPERIMENT XIV.

1. **W**HEN I had seen to what Height the Blood would rise in Tubes fixed to the carotid Arteries of several Dogs, then taking away that Tube, I immediately affixed to the Pipe that was inserted into the Artery another Tube, which was four Feet and half high, to the middle of the Tunnel which was fixed on the top of it: Then cutting open both the jugular Veins, blood warm Water was poured into the Tunnel, which flowing down thence from a perpendicular Height equal to that of the arterial Blood in the former Tube, was by this means impell'd thro' all the Arteries of the Body with a Force nearly equal to that with which the Blood was propelled by the Heart, and was thence carried with the venal Blood out at the Jugulars; where the flowing Blood became more and more diluted with Water, till the Dog died; after which very little Water would pass out at the jugular Veins. When the Column of Water in the Tube was $9 + \frac{1}{2}$ Feet

Feet high, the Blood then flowed more briskly out at the Jugulars.

2. The Dogs constantly died when the Blood grew very dilute with Water; whence we see the meer keeping the Arteries full with any Fluid will not support Life: No wonder then that the Lamp of Life burns more and more dimly, and is in further and greater danger of being extinguished, in proportion as that noble vital Fluid the Blood, becomes more and more depraved.

3. It was observable that the Dogs constantly expressed great Uneasiness instantly as soon as the blood warm Water entered the Arteries, and mixed with the Blood. Hence if our Drink entered the Blood-vessels at once it would be most pernicious; but Nature gradually assimilates it, by first mixing it with the Chyle, and other Juices, which are in great Plenty secreted from the Glands of the Stomach and Guts, as also from the Liver and Pancreas, and from the mesenteric Glands; which Commixture and Assimilation is incessantly carrying on by infinite Mixtures in the circulating Vessels, without which the Blood could not be diluted, but its strongly attracting Globules would soon coalesce.

4. This warm Water, thus mixed with the Blood, did usually make the Dogs vomit, especially when the Column of Water was $9 + \frac{1}{2}$ Feet high ; whence we see that warm Water in the Blood has the same Effect, in giving a convulsive Motion to the muscular Fibres of the Stomach, as when taken inwardly, in which case it is well known to cause a *Nausea* and Vomiting ; a probable Argument that some of it then soaks immediately out of the Cavity of the Stomach among its muscular Fibres.

5. And it has the like Effect upon all the Muscles of the Body ; for when the Water enters the Muscles two or three Minutes after the Dog is manifestly dead, then all his Muscles are in strong Convulsions, and that for some Minutes.

6. If the warm Water was continued thus flowing into the Artery for half an Hour, or two Hours, all the Parts of the Body would during that time, be continually swelling bigger and bigger, so that there would be a universal Dropsy over the whole Body ; both the *Ascites* and the *Anasarca* : The salival and other Glands were greatly swell'd, and the Mouth and Nose filled with muscose slimy Matter which

which flowed from those Glands; the *Ubera* were much distended by the filling of their fat Vesicles, as were also all the fatty Vesicles of the Body. All the Muscles were swelled and the Interstices of their Fibres filled with Water; and some of them were by this means washed white. All this was effected with a Force of Water no greater than that of the arterial Blood in its natural State.

7. Whence 'tis probable that no Vessels were broken to make way for this universal Inundation; but the penetrating Water might readily pass thro' Pores, and such fine secretory Ducts, as the more viscid Part of the Blood in its common Course of Circulation never enters; but thro' which its more attenuated and diluted Parts do in their due Proportion pass. Thus for instance we see when the penetrating Water flows freely into the secretory Ducts of the salivary Glands, it makes the *Saliva* flow plentifully thro' the excretory Ducts, which in a natural State being more gradually separated from the Blood flows also more leisurely thro' those Ducts.

8. But when the Tube, thro' which the Water flowed into the Arteries, was $9\frac{1}{2}$ Feet high, then such a Force would sometimes

drive some little Blood thro' the salival Glands, and among the fat Vesicles of several Parts of the Body, as also into the Cavity of the Guts; which would thereby be distended full of Water, if the *Abdomen* was open; but when that was not opened, tho' Water continued for a hundred Minutes to flow in at the carotid Artery, so that all the Parts of the Body were exceedingly swelled, and there was Water also in the Cavity of the *Abdomen*; yet there was not half so much Water in the Cavity of the Stomach and Guts, as there would have been, if the *Abdomen* had been all that time open. Hence we see how the great Compression of dropical Humours on the Blood-vessels and secretory Ducts may retard the natural Secretions of Humours into the Bowels and thro' other Glands; whereby the Blood must needs become more and more depauperated, for want of a sufficient Supply of those Secretions, to mix with, and assimilate the Chyle gradually into Blood. Hence also from the Stoppage of the Secretions of the salival Glands, the constant Thirst of the Hydropic.

9. Frequently, when the Dog was dead by washing his Blood out in the manner described,

scribed, *Numb.* 1. of this Experiment, I used then while all was warm, immediately to open his Abdomen and Thorax, and then fix the Tube to the descending *Aorta* a little below his Heart: This large Artery admitting a brass Pipe of a larger bore, the Water flowed more freely in; which it continued to do for more or less time, according to what Experiments and Observations I intended to make. And while the Water thus flowed thro' the Arteries, I kept all parts of the Dog warm, by pouring Water on him, and then laying a Cloth dipped in warm Water over his Body, and sometimes by placing him in a Vessel of Water.

10. Tho' this Pressure of Water was equal to the Force of the arterial Blood, which had been washed out in killing the Dog, yet none of the warm Water passed thro' the Kidnies into the Ureters and Bladder, tho' the Kidnies were distended to Hardness: Nor would any pass thro' the Blood-vessels of the Bladder into its Cavity, whence 'tis probable that it has no Lymphaticks at least within side: Yet the Blood-vessels of the Coats of the Bladder were filled with Water, as is evident, because they were constantly on Injection well filled with the tinged Liquor. A probable Argument

that no Urine is thence separated immediately from the Blood into the Bladder; and consequently there can be no other Passage for the Urine into the Bladder than thro' the Kidnies and Ureters: The easy Passage of Fluids from the Stomach and Guts, immediately into the mesenteric Veins, and the great Velocity of the Blood in circulating, may rationally enough account for the quick Effects, that some Fluids have on the Urine soon after drinking.

11. The Liver from its own natural Redness and Degree of Firmness, turned gradually less red, and sometimes whitish, but always swelled and was very hard; the Water did not flow thro' its Vessels into the *Vena Cava*; The Gall Bladder was constantly distended very full, so as to flow into the Guts. The Pancreas was full of Water as also the Spleen, which was seldom much distended, but was sometimes washed so clear of Blood as to be very well injected with tinged Liquors.

12. I slit open four or six Inches Length of a Gut, just opposite to the Insertion of the mesenteric Vessels into the Gut: And having then so folded open the Gut as to have the inward mucose Coat outwards, I wiped it dry
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with a Sponge, which was soon moisten'd in every part again, by the Water which ouzed thro' from the mesenteric Arteries ; and it was the same on frequent Repetition, and also when I held this open part so as to make a small Cavity or Basin with it, it would then also be soon filled with Water.

13. In another Dog whose Gut was not slit, the Quantity of Water which flowed in, was in some Continuance of time so great as to distend the Guts and even burst the Stomach. Whence we see how easily the thinner Part of the Blood may flow into the Cavity of the Stomach and Guts, as it is found in Fact to do in living Animals : And as such large Quantities are thus secreted into the Cavity of the *Viscera*, this must needs have a great Influence on the Blood, when these Secretions are either too plentiful and thereby disorder it by too great Evacuations, or when they are on the other hand too small, or are too suddenly stopped, the ill Effects of which are soon felt in the Head and Lungs, whence often a Fever.

14. It is common for habitual hard Drinkers to labour under the Inconvenience of too great Discharges of these Humours into their Stomachs,

Stomachs, which is occasioned by their continually swallowing down too great Quantities of Liquor, with which the Blood being furcharged, must needs make those more than natural Discharges of it into the Stomach, which they usually complain most of in a Morning. Whence readily inferring that they have a cold watry Stomach, they seldom fail of warming it again soon, by so agreeable a Prescription, as a large Dose of the same Liquor; sagaciously concluding with the poor unhappy *Minute Philosophers*, that because it is most agreeable to depraved Nature, therefore it must be best; tho' in reality it does necessarily increase the Distemper.

15. While the Tube was thus fixed to the descending *Aorta*, and Water continued flowing thro' it, I cut the *Vena Porta* asunder, which carries the Blood from the Stomach and Guts thro' the mesenteric Veins to the Liver: The much diluted Blood which was contained in it, for want of a free Passage thro' the Liver, rushed forcibly out of the Vein; but afterwards the Water which came from the mesenteric Arteries, flowed out of the *Vena Porta*, at the rate only of half a cubic Inch in forty Seconds of Time, for

want of a free Passage from the Arteries to the Veins.

16. When I fixed the abovementioned Tube to the *Vena Porta* of another Dog, so as to make the Water pass thro' it to the Guts, having then slit open a part of a Gut as before describ'd, *Numb.* 12. I found the Water ouzed plentifully thro' the whole mucose Coat into the Gut; whence we see there is a Passage for the Chyle to pass immediately from the Cavity of the Guts into the mesenteric Veins.

17. Yet when *e contra* a Tube was fixed to the Cavity of a Gut and warm Water poured in, no Water would pass thence into those Veins, tho' the Column of Water which pressed to enter, was of several Lengths from one to $9\frac{1}{2}$ Feet high; the Entrance of the Water being hindred by the connivant Valves which cover the Orifices of those capillary Veins, which opening obliquely into the Intestines, their Orifices are compressed by the incumbent Water: Were it not for this wise Precaution, very gross and noxious Particles of the Contents of the Bowels would have been impelled thro' these Veins and the lacteal Vessels, into the Habit of the Body, and that in greatest Abundance, when the Guts were
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most distended either with Ailments or Wind. Now the Force of the Blood in the Veins being not above $\frac{1}{10}$ th or $\frac{1}{12}$ th of that of the Arteries, and their Number and Capacities being also much greater, they are thereby more aptly disposed to imbibe Chyle from the Intestines, whose peristaltic Motion, together with the alternate Dilatations of the Arteries, and reciprocal Compressions, Dilatations and Relaxations of the Diaphragm and abdominal Muscles do greatly promote its Progress. But when thro' any Obstructions the free Course of the Blood thro' the Liver is retarded, it being necessarily thereby more accumulated in the mesenteric Veins and *Vena Porta*; the imbibing Power of those Veins from the Intestines will not only be proportionably rebated; but also the Velocity with which the Blood should pass along the mesenteric Arteries, thro' the Coat of the Guts, being thereby retarded, will subject the Bowels to many Disorders.

18. It seems evident from this XIVth Experiment, that the Secretions, which are different according to the different Texture of the secretory Vessels, and which are made from the arterial Blood thro' Vessels finer than the finest

finest circulating Arteries, are not made *plengurgite*, that is, with the full Force of the arterial Blood ; for if they were, then all the secretory Vessels and Glands would swell as they do in this Experiment with Water ; as they do also in dropical Cases, when the redundant thin Fluid is separated too freely from the Blood. These Secretions must therefore be made more gradually and sparingly, so as to be carried forward in those very fine Vessels, by an alternate pulsive Force of the arterial Fluid, and attractive Power of the fine secerning Vessels ; assisted also by constant Vibrations, for the animal Fluids and Solids are in an incessant mutually vibrating State. In this manner doubtless the plentiful Secretions are made into the Stomach and Guts, as also in the Pancreas, mesenteric, salival and other Glands of the Body. And thus also the perspiring Matter is carried off, not alone by the meer protrusive Force of the arterial Fluid, but also by the Warmth and mutually vibrating Action of the Fluids and Solids. And when by Labour or other brisk Exercise, the Velocity of the Blood is increased, and consequently its Warmth, then not only its Force, but the Vibrations also of the Fluids
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and Solids being thereby increased, the Perspiration is consequently also so much increased, as to pass in the visible Form of Sweat thro' the Pores, which by *Experiment XV, Numb. 9.* are dilated by Heat.

EXPERIMENT XV.

1. **T**HIS hydraulic and hydrostatical Method of examining the animal Canals, is not only of use to shew the Force of the Blood in the Veins and Arteries, the great Resistance it meets with in its Progress thro' the finer capillary Vessels, and many other things which are shewn and deduced from the foregoing Experiments: But may also be very serviceable in shewing what Effect different Liquors have on the finer Vessels of the Body, *viz.* when they are either hot or cold, or have different Degrees of restraining or other Qualities.

2. For since the healthy State of an Animal consists principally in a due Equilibrium between its Fluids and Solids, so that an Error in the Solids will greatly contribute to the disordering of the Fluids too, it will therefore be very useful to see, what Effects different

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ent Liquors have on those Solids, either in relaxing them when too tense, or in contracting and strengthening them when too lax. And tho' these several Properties of many of them are by long Experience well known to the skilful Physician, yet since it is making some Advance in Knowledge, farther to illustrate even known Truths, a fuller Demonstration of the Effects of some of these Liquors may be of Service in confirming the known Explinations of the Operations of Medicines, and in farther clearing up the Reasons of such as are more doubtful.

3. I took a young Spaniel Dog which weighed twenty one Pounds, and as soon as he had bled to Death by having his jugular Veins cut, I immediately opened his *Thorax* and *Abdomen*; and having fixed a glass Tube, which was $4\frac{1}{2}$ Feet high, to the descending *Aorta*, I then slit open his Guts from end to end in the same manner as in *Experiment IX*; then having poured blood warm Water on them, and covered them with a folded Cloth dipped in the same Water, warm Water was poured into the Tunnel, which when it had subsided to a Mark on the lower Part of the glass Tunnel, eighteen cubic Inches of warm Water were
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immediately poured in, out of a Pot which held just that Quantity: The Time that it was running thro' the fine capillary Arteries was measured by a Pendulum that beat Seconds.

4. I first poured in seven Pots full of warm Water, the first of which passed off in fifty two Seconds, and the remaining six, gradually in less time, to the last which passed in forty six Seconds.

5. Then I poured in five Pots of common Brandy, or unrectify'd Spirit of Malt, the first of which was 68" in passing, the last 72".

6. Then I poured in a Pot of warm Water which was 54" in passing.

7. Hence we see that Brandy contracts the fine capillary Arteries of the Guts, and that Water soon relaxes them again, by diluting and carrying off the spirituous Part of Brandy, which as it is well known, not only contracts the Coats of the Blood-vessels, but also thickens the Blood and Humours, both which Effects contribute to the sudden Heating of the Blood, by much increasing thereby its Friction in the contracted capillary Vessels; which sudden Heat is also further increased by the mere Mixture of Brandy with the Blood as

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Dr. *Boerhaave Elementa Chemiæ*, Vol. I. p. 366 observed, that on the mixing of cold Water and Spirit of Wine, they immediately acquired eight Degrees of Heat, so as to make the Mercury in *Fahrenheit's Thermometer* rise from forty four to fifty two Degrees; and sometimes in a like Mixture it has risen to fifty three Degrees, which Heat soon ceased, as does also that sudden glowing Heat which it gives the Blood. Hence it is that the unhappy habitual Drinkers of Brandy and other distilled spirituous Liquors, do so insatiably from time to time thirst to drink of that deadly Liquor, which by often heating the Blood and contracting the Blood-vessels, does by degrees reduce them to such a cold, relaxed and languid State, as most impetuously drives them to seek for their Relief in that Liquor, which they too well know both by their own Experience, as well as by the daily Destruction of Thousands, to be so very baneful and deadly, as to become by the great Abuse of them the most epidemical and destructive Plague that ever beset Mankind.

9. When cold pump Water which was fourteen Degrees above the freezing Point, was poured on the Guts, and some of it at

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the same time poured thro' the Tunnel into the Arteries, those capillary Arteries would on a sudden be so contracted, as that the fourth Pot of cold Water was 80" longer in passing thro' than the like Quantity of warm Water was just before: And the fifth Pot being warm Water, and the Guts at the same time being warmed by Water poured on them, it passed in 77" sooner than the former Pot of cold Water did.

10. Hence we plainly see how greatly Heat and Cold dilate and contract our Pores, which must therefore consequently have a proportionable Effect on our Perspiration, that great and important Evacuation: Thus hot Baths greatly increase Perspiration, but a cold damp Air, and cold North-easterly Winds, by constringing the Pores retard Perspiration, notwithstanding there is no Abatement of the internal Heat: And on the other hand, when the Blood is cold, as in Dropsies, the Perspiration will be much rebated for want of a due internal Heat, notwithstanding the Pores may be relaxed: yet in a burning Fever, when by reason of its great Heat, the Pores should be open, there is notwithstanding very little Perspiration; this as well as the other glandular Secretions being
greatly

greatly diminished and in a manner obstructed, by the then viscid State of the Blood.

11. When immediately after warm Water, three Pots of Water, so warm as that I could scarcely bear my Hand in it, were poured into the Arteries, the third Pot passed thro' in less time by 30", than the preceding warm Water; and the next Pot of Water which was considerably hotter than the last passed off sooner by 18" than the foregoing. The like hot Water was at the same time poured on the Guts.

EXPERIMENT XVI.

1. **I** Made a strong Decoction of Peruvian Bark, boiling a Pound of it in three Gallons of Water to two; when cold it was filtrated several times thro' a Flannel Bag. The Day following I prepared and slit the Guts of a young Spaniel Bitch as in the foregoing Experiment.

2. I first poured into the Tube which was fixed to the *Aorta* four Pots of warm Water, containing eighteen cubic Inches each, the last of which passed off in 62" of time. Then I poured in successively sixteen Pots of equally warm Decoction of Bark, the first of which passed off in 72" and the following slower

and slower, in proportion as the Vessels grew more and more contracted by the stiptic Quality of the Decoction, so that the 16th Pot was 224" in passing.

3. Then I poured in successively eleven Pots of Water of the same Heat with the Decoction, the first of which Pots was 198" in passing, and the succeeding ones passed sooner and sooner in proportion as they washed off the Decoction, and thereby relaxed the Coats of the capillary Vessels, till the eighth Pot which passed in 96", after which the three following Pots of Water passed off in the same time, the Vessels relaxing no farther. It was not to be expected that the Water should so far relax them as to pass off in 62" of time, which the abovementioned fourth Pot of Water did in this Experiment; for I always found that by a long Continuance to pour in Water, the Vessels did gradually let less and less pass; the capillary Vessels being compressed by the Water that insinuated into all Parts of the Coats of the Guts, so as to make them much thicker than they were at first; now the Constriction which is thus occasioned can only be enlarged by the Affusion of much hotter Water, as was done in the foregoing Experiment; whereas

whereas the Constriction of the Vessels which is owing to the stiptic Quality of the Liquor, is we see, in this Experiment and others, manifestly taken away by the relaxing Quality of the Water, which washes away the stiptic Vertue.

4. I then poured in successively five Pots of cold pump Water which was fourteen Degrees above the freezing Point, and whereas the preceding Pot of warm Water passed in 96", the fifth Pot of this cold Water was 136" in passing.

5. I tried in like manner a strong Decoction of Oak Bark on another Dog, in which the preceding Pot of warm Water was 38" in passing, but the following six Pots of Decoction so contracted the Vessels that the last of them was 136" in passing.

EXPERIMENT XVII.

1. **H**AVING prepared a Decoction of twelve Ounces of Chamomel Flowers, boiled in three Gallons of Water to two, I poured it blood warm thro' the Arteries of the slit Guts of a large Pointer Spaniel. I found by the Velocity with which the four first Pots

of Decoction run off, that there was by Accident a large Branch of an Artery cut; which being stopped by a Ligature, I poured in successively eleven Pots of the Decoction, the first of which passed off in 96" of time, the last in 138"; so that there is some Degree of Stipticity in this Decoction. Through inadvertency I omitted to pour in first some Pots of warm Water, by which the Degree of Stipticity would have been known more nearly.

2. Then the last of four Pots of scalding hot Water passed in 116".

3. After these, six Pots of a Decoction of two Ounces of Cinnamon in a Gallon of Water being poured in hot, the Vessels gradually contracting, the last Pot was 216" in passing. We see in this Instance how effectual Cinnamon by its great Stipticity is in stopping too large Discharges into the Cavity of the Guts.

4. Then a Pot of milk warm Water passed in 15".

5. Next a Pot of scalding hot Decoction of Chamomel Flowers was 194" in passing; which further shews its Stipticity.

EXPERIMENT XVIII.

1. **H**AVING as in the three foregoing Experiments prepared and slit the Guts of a Dog, I first poured twelve of the above-mentioned Pots full of warm Water thro' the Arteries, the first of which was 68" in passing, the following Pots passed successively faster and faster, to the four last which all passed off in 38".

2. Then I poured in seventeen Pots of equally warm *Piermont* Water, the first of which was 40" in passing, and the following Pots were in Succession longer and longer in passing to the 17th which was 76" Seconds in passing.

3. I then poured in ten Pots of equally warm Pump Water, which gradually relaxing the capillary Arteries again, each Pot passed off a little sooner and sooner, to the last, which was 64" in passing off.

4. We see in this and the three foregoing Experiments, how the Vessels of the Body are manifestly contracted or relaxed, by different Degrees of Warmth, Heat or Cold, or from the different Qualities of the Fluids which

pass thro' them, as to Restrictingency or relaxing : And such Qualities of the Fluids must have very considerable Effects on the finer capillary Vessels, whose Coats bear a much greater Proportion to the contained small cylindrical Fluid, than in larger Vessels. Tho' it is not to be imagined that the Effects are so sudden and great in a live Animal, as in these Experiments ; because in a live Animal, the several Fluids which are taken in, are more gradually and in smaller Proportion blended with the Blood.

5. 'Tis probable that such things as constrict the Vessels in any Degree, do also proportionably increase the Force of the arterial Blood, and thereby invigorate the Animal. For since the more the extrem capillary Vessels of the Arteries are contracted, so much the greater Force will be requisite to impell equal Quantities of Blood thro' in equal times ; the Blood must needs therefore be the more accumulated in the Arteries ; and being thus impelled thro' smaller capillary Vessels with greater Force, it will thereby both acquire a greater Degree of Heat, and be also the more attenuated and digested. 'Tis by this means that Bitters, such as Chamomel Flowers and the Peruvian Bark may

may be greatly beneficial to the Blood, as well as by rectifying its *Craſis* in mixing with it as a Menſtrum. Thus the *Cortex* is in a double Capacity an Attenuator, both by reſtringing the Veſſels, and alſo by diluting as a Menſtrum the Fluid of the Blood, which it is well known to do, when mixed with extravafated Blood: And in like manner Calybeats which are ſtiptic, do attenuate the Blood; thus alſo roſy Wines are cured by ſtiptic Attenuators, which precipitate the Tartar.

6. The ſudden glowing Warmth that Brandy gives, is owing not only to its raiſing a Warmth in mixing, as was before obſerved, with the Blood as a Menſtrum, as it does when mixed with cold Water; but to this alſo, that at the ſame time that it gives a contractive Spring to the Coats of the Veſſels, it alſo thickens the Blood, which muſt neceſſarily cauſe a much greater Reſiſtance and Friction between the contracted Veſſels and the more inſpiſſated Blood, which muſt needs therefore raiſe a greater Heat: Whence the Blood-veſſels in the Brain being dilated by theſe or other ſpirituſous Liquors, and thereby too plentiful Secretions being made into the Subſtance of the Brain cauſe Intoxication and Sleep. Where-

as the *Cortex* by attenuating the Mass of Blood, at the same time that it contracts the Vessels, does not occasion that sudden glowing Heat that Brandy does, notwithstanding it may perhaps equally contract the Vessels. But when the Bark is given in the Paroxysm of a Fever, it is observed to increase and prolong the Fever, which is probably owing to its restringing Quality, which by lessening the Diameters of the Vessels, in that too rapid State of the Blood, must necessarily increase its Heat.

7. It is by this Restringency also that the Bark is supposed to cure those who are subject to profuse Sweatings, it contracting the Pores.

8. The contractive Effects of different Liquors on the Vessels may be of very different Length of Duration; thus that of Brandy is of short Duration, for vinous Spirits are easily totally absorbed by a more watry Fluid; whereas the Effects of other Restringents on the Solids, such as *Cortex*, *Chamomel*, &c. *Piermont*, *Spaw* and other calybeate Waters are of a more permanent Duration. But those who much accustom themselves to drink strong spirituous Liquors, do thereby destroy the Tone of the Fibres of their Vessels, by having
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them thus frequently, suddenly contracted, and so soon relaxed again ; which make them like the Horse Leech, be ever longing after and thirsting for more and more, thereby to regain the Tensity of their too relaxed Fibres.

9. Thus what we take in, either as Food or Physick will have different Effects on our Solids as well as Fluids, according to their different Natures ; and as a healthy State consists in a due Equilibrium between the Solids and Fluids, it greatly imports, that they be rightly adapted to the different Constitutions, so as either to invigorate, contract or relax the Fibres of the Solids ; or to change the Qualities or Quantities of the Fluids as occasion shall require.

EXPERIMENT XIX.

1. **T**HAT I might know with what determinate Force I impelled Air into the Vessels, I prepared the following Instrument, *viz.* I fixed to a common brass cylindrical Air Force an Elder Stick, which was two Feet long, and two Inches Diameter ; thro' which a Hole of half an Inch Diameter was made from end to end : One end of an inverted Glass Syphon

Syphon was laterally fixed into a Hole in the middle of the Elder Stick ; then four Inches Depth of Mercury being poured into the Syphon, its other Orifice was closed up with Cement and a Piece of Bladder tied over it. When this Instrument was by means of a brass Pipe fixed to any Vessel of an Animal, I could by the Height of the Mercury in the mercurial Gage see with what Force Air was impelled.

2. When Air was with this Instrument forced into the descending *Aorta*, or the *Vena Porta*, none passed thence into the Cavity of the Guts, tho' Water by *Experiment XIV. Numb. 12.* did freely.

3. Neither when I slit a part of the Guts lengthways, would any Air pass thro' those transversely cut, converging Arteries, when impelled with a Force equal to that of the arterial Blood: But when those Arteries had been first washed, by the Flowing of warm Water thro' them, then the Air also would pass freely ; which shews how requisite it is to wash the Blood-vessels, before Injections be made with coloured Liquors.

4. Tho' Air when blown in at the *Aorta* will not thence enter the Cavity of the Guts,
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yet it will pass thither when it lays in a latent State in the Interstices of a Liquor : For having cut asunder in two Places a Dog's Gut, at the Distance of two Feet Length, I washed that Piece of Gut clean, by passing of warm Water thro' it ; then tying up each end of it, new frothy small Beer which was blood warm, flowed from a perpendicular Height of $4\frac{1}{2}$ Feet thro' the descending *Aorta*, and thence after some time, in considerable Quantities into the Stomach and Guts. I found a Quantity of it equal to two cubic Inches in the Piece of Gut which was washed and tied up at each end ; it was turbid and of a tawny Colour like the Grounds of Beer. When I warmed this again over the Fire, fresh Froth arose on the Top of it : An Argument that the flatulent Air which is generated in the Stomach and Bowels, may not arise wholly from flatulent Food, or some Irregularity in the Digestion of it ; but that it may in part also arise from too flatulently disposed Secretions that are made into the Stomach and Guts, from the Glands of those *Viscera*. If therefore there be at any time Air in the Blood-vessels, it may, by being first resorbed into the Blood, be by this means discharged from it in these plentiful Secretions.

EXPERIMENT XX.

1. **W**HEN Tubes were fixed at the same time to the left carotid Artery, and also to the left crural Artery, and Water poured into the Tube at the carotid Artery, so as to be four Feet seven Inches high in that Tube, then it would rise four Feet four Inches in the other Tube: And when the Water subsided six Inches in the carotid Tube, it would proportionably sink in the other Tube and rise again when that was filled up. When the Column of Water in the carotid Tube was $9\frac{1}{2}$ Feet high, then it rose in the other Tube eight Feet eleven Inches. The Inequality of the several Heights of the Columns of Water, was as I guess owing to the Smallness of the brass Pipe which was fixed to the carotid Artery, on which Account it was not sufficient to supply in full Plenty what ran off to waste in the other Branches, and at the same time keep up the Water near to an equal Height in the other Tube, which it would doubtless have done, if by a larger Pipe the Tube had been fixed to the descending *Aorta*.

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2. When I took away the Tube from the crural Artery, and the Water at the same time stood $9 + \frac{1}{2}$ Feet high in the other Tube, then the Water spouted $11 + \frac{1}{2}$ Inches out of the Artery which was held with its Orifice upright.

4. When a Column of Water was $4 + \frac{1}{2}$ Feet high pressed into the carotid Artery, then another Tube being fixed at the same time in the *Vena Porta*, pointing towards the Guts, the Water rose slowly six Inches in the Tube: But when the Tube was fixed in the descending *Vena Cava* towards the Dogs Legs then no Water rose in it.

4. If we could be so happy as to find a Liquor of such a due Consistence, as to pass freely thro' from the Arteries to the Veins, as the Blood does in its natural State, then many curious and useful Experiments might in this manner be tried on several Parts of the Body.

5. It was with this View that I made Columns of warm Water flow into the Arteries of dying Dogs, at the same time that the Blood run out at the Veins, *viz.* to try if by this means I could wash all the glutinous Blood out of the finer capillary Arteries and Veins, by making Water thus instantly follow
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and impell the Blood out of them: But I was soon disappointed in my Expectations; for the Water tho' a much more dilute Fluid than the Blood, had no free Passage from the Arteries to the Veins; the reason of which seems to be, that, as appears in many Instances in the second and third Chapters of *Vegetable Staticks*, Water entering the Interstices between the Sap-vessels of the cut Branches, did in a few Days compress the Sap-vessels, they withal at the same time contracting themselves, so that no Water could enter them: And the Case is just the same here in Animals, for Water in many of the foregoing Experiments, freely passing from Arteries thro' innumerable Passages, which were too fine for the Blood Globules to enter, does thereby as in Vegetables, so compress the extream capillary Arteries, as to hinder a free Passage thro' them; and this is further confirmed by the Observation, *Numb. 3. Exper. XVI.* where it was observed, that the larger converging capillary Arteries of the Guts were gradually more and more compressed, by the Continuance of Water flowing thro' them.

6. We may hence also with good reason conclude, that the extream capillary Arteries,

are of a more elastic contractive Nature, so that the Water, not being like the Blood full of Globules, it cannot keep those Vessels open, as the Blood does by an uninterrupted Series of Globules following each other ; and the greater Proportion the Quantity of these Globules bear to the more diluted part of the Blood, the less Quantity of Fluid will be secerned from the Arteries and *Vice Versa*. And hence it is that it is found so difficult to inject with Vermilion or other coloured Liquors, the immediate Communications between the Arteries and Veins, in pursuit of which I made some few Attempts, *viz.*

EXPERIMENT XXI.

I. **T**H O' I was sensible that anatomical Injections have of late Years been carried to a good Degree of Perfection by several skilful Anatomists ; yet as they both prepared their Vessels by injecting Water into them with a Syringe, and also injected their melted coloured Liquors by a Syringe ; in using of which Instrument, one cannot be assured, with what Force either the Water or melted Liquor is impelled, so as to be secure from breaking the finer Vessels, or from making Blotches by

too strong an Injection ; therefore, to remedy these Inconveniences, I thought it would be safest, as well as more effectual, to wash the Blood-vessels from a perpendicular Height of Water, which should impell it with a Force no greater than that of the arterial Blood : And after the Vessels were thus cleansed, to make the Injections of coloured Liquors, by pouring them thro' hot Iron Tubes of such Lengths as Experience should prove to be best : This I hoped might be a likely Method to bring the Art of injecting to greater Certainty and Perfection. But tho' I have not succeeded herein so well as I expected, yet it may not be improper to give a short Account of some few Attempts which I made in this Way ; that more skilful Anatomists may judge whether they shall think it adviseable to pursue the Matter further ; in which Pursuit I am persuaded they would not lose their Labour.

2. When I intended to inject coloured Liquors into the Blood-vessels of a Dog, having fixed a Tube $4\frac{1}{2}$ Feet high to his left carotid Artery, and then cut both jugular Veins open, I immediately poured blood-warm Water down the Tube, to wash the Blood out of
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the Vessels: Then as soon as the Dog was dead, I opened his *Abdomen*, if I designed to inject the lower Parts, and cut open the *vena Porta*, and descending *Cava*, to give room for the Water to wash the Blood from the corresponding Arteries into them; for the Blood having no Passage in the dead Dog, thro' the Liver, it was so pent up in the *Porta*, as to rush out forcibly when that Vein was cut. Then the Water continued to flow thro' the Arteries from the Tube; which was supplied either from a large Vessel of warm Water whence it run, or else by lading it into the Tunnel; this was continued for half an Hour, and sometimes an Hour or more, till the Guts and Stomach were white.

3. Then opening the *Thorax*, I fixed a brass Pipe to the descending *Aorta*, and to the Pipe was fixed to a Gun Barrel, which was heated by pouring boiling Water into it, several times, and with which it stood filled till wanted, its great End standing also some Depth in a Vessel full of hot Water: Then the injecting Liquor being duly heated, was poured down the Barrel till it was full, thro' an Iron Tunnel which was fitted to the Outside of the Barrel, that it might have a larger Orifice for Liquor to flow in fast.

4. I had at first no brass Cock at the Bottom of the Barrel, but being apprehensive that the Liquor which first flowed into the Arteries, before the Barrel was full, might not go to the Extremities, for want of a sufficient Impetus from a greater perpendicular Height, I therefore fixed a brass Cock at the bottom of the Barrel, which being stopped till the Barrel and Tunnel was full, was then immediately opened to let the Liquor rush into the Arteries with a full *Impetus*. This Method did sometimes succeed pretty well, but sometimes no better than the other, which made me suspect, that the Vermilion might by its great specific Gravity, subside in too great Quantity towards the Bottom of the Barrel, before the Cock was opened.

5. One of the Barrels was $4 + \frac{1}{2}$ Feet, and the other $5 + \frac{1}{2}$ Feet long; I sometimes used them singly, and sometimes joined together in one Tube; this Column of ten Feet never caused any Blotches in the Injection.

6. The Mixture I made use of was the following, *viz.* white Rosin and Tallow of each two Ounces, melted and strained thro' Linnen, to which was added three Ounces of Vermilion, or finely ground Indigo, which was first
well

well mixed with eight Ounces of Turpentine Varnish. I was obliged to Mr. *Ranby* for communicating to me this Receipt.

7. I kept the Bowels warm, either by frequently pouring warm Water on them, and covering them with a wet Cloth, on which warm Water was often poured, or else by immersing the whole Dog in a Vessel of warm Water. I believe I should have succeeded better if this Water had been as hot as that mentioned under *Experiment XV. Numb. 11.* for that Degree of Heat, as we there see, not only disposes the fine Vessels to dilate much, but coming also near to the Heat of the melted injected Liquor, it will not cool so soon, whereby it will have more time to insinuate itself into the finer Vessels; which Insinuation is greatly promoted by the constant even Pressure of the Column in the Barrels; for which Reason I did not take away the Barrels till all was cold.

8. I was in hopes by this means to have fixed the coloured injected Liquor, in the immediate Communications between the Arteries and Veins, but could not succeed so well as I expected to have done, notwithstanding the Injection passed from the Arteries into

the Veins of the Stomach, Guts and Urine-Bladder, but especially in the Gall-Bladder; and carried with it, sometimes a little Vermilion and sometimes none. I have a Gall-Bladder which was injected from a Column $4 + \frac{1}{2}$ Feet high, without a brass Cock at the Bottom of the Barrel, all the Blood-vessels of this Dog had first been washed for an hundred Minutes, from a Column of Water $9 + \frac{1}{2}$ Feet high: The Arteries and Veins of this Gall-Bladder are both injected, and there is a considerable Quantity of Vermilion in the Veins, tho' not so much as in the Arteries; I can plainly see with a Microscope here and there an extream Artery injected to the very Coat of the Vein, which it enters at right Angles; so that the Blood circulates by immediate *Anastomosis* of the Arteries and Veins, without the Interposition of any glandular Cavities.

9. The immediate Communication between the Arteries and Veins seems to be in this manner, *viz.* the Arteries which converge and inosculate into each other, send out of each of their converging Sides Branches at right Angles to those Sides, which Branches soon divide, like the spread Fingers on the Hand, into several much finer Branches; and these into others,

some fewer, some more, according to what part of the *Areola* or Net-like Mesh they are to reach to; hence they dip at once, at right Angles down to the Veins, which they enter also at right Angles, some into the larger converging Veins, others into the lesser Veins, which like the Arteries branch at right Angles from the converging Veins, and spread like them the *Areolas* or Meshes: But the *Areolas* between the converging Arteries are much larger, and more nearly right sided and rectangular than the *Areolas* of the converging Veins, which approach nearer to Circles.

10. The great Disproportion there is, between the Force of the arterial and venal Blood, shews how needful it was, not only to have all the Communications between the Arteries and Veins so very small that only single Globules can pass; but also to have the Blood both pass from the Arteries at right Angles, and also to enter the Veins in the same manner, which must greatly contribute to the retarding of its Motion, especially in very fine Channels. Else the arterial Blood would rush with such Rapidity, as thereby to make the Force of the Blood in the Arteries and Veins come nearer to an Equilibrium; the obvious Conse-

quence of which would be, that the Blood could not be impelled thro' the finer capillary Vessels, besides many other Inconveniences that might be enumerated. But the Extremities of the Arteries are not in all Parts alike, for in some Places they do not form a Network; so that the Passage of the Blood from the Arteries may be in a very different manner in different Places.

II. Since from *Experiments* XV and XVI, &c. it is found that where Water has long passed thro' the Arteries, it is apt to contract the Vessels by dilating the adjoining Parts; and since in *Experiment* XXIII, *Numb.* 7. it is observed that when the Coats of the Stomach have been thus filled with Water, on blowing up the Stomach that Water has flowed out plentifully from those Coats of the Stomach; it should therefore seem a good Method, to distend the Stomach and Guts with Wind for some time, in hopes to get out a good Part of the Water which was lodged in their Coats, while the Blood-vessels were washing clean, taking care to immerse the Dog all the while in warm Water. 'Tis probable that by this Means the Blood-vessels would be well cleansed and emptied, and thereby rightly prepared
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to make a good Injection. And if one of the crural Arteries were opened and had a Ligature put round it, which Ligature might be instantly tied as soon as the injected Liquor had forced the Water and Air out of the *Aorta*, which must else have been drove before the injected Liquor into the finer capillary Vessels, which doubtless often hinders the Injection from penetrating so far as it would otherwise do.

12. Some of the Vermilion came always into the Cavity of the Bowels, tho' the Injections were made with no greater Force than that of the arterial Blood, *viz.* with a Column of $4 + \frac{1}{2}$ Feet Height; and it was the same whether the Injection were made by the *Aorta* or *Vena Porta*, for in both cases, the red Streaks of Vermilion might be seen with a Microscope in the *Papillæ* of the mucose Coat of the Bowels.

13. As none of this Vermilion passed into the Lymphaticks, fat Vesicles, or extravasated Parts, as the Water did, this is a Proof that the Water which was impelled with no greater Force than the Vermilion, did not burst any Vessels, when it became extravasated, but that it passed thro' the finest discerning Tubes, as also thro' the Pores of the Vessels, where viscid Blood cannot pass.

14. I washed the Blood-vessels of a Dog with ten Quarts of Water, in which five Ounces of Nitre had been dissolved, to try whether this Liquor would more effectually cleanse the Vessels, but it had the contrary Effect; for all the Parts of the Body continued very red, after long washing, so that the Nitre which attracted strongly the more sulphureous red Globules, fixed them in all the Vessels. It was very remarkable that none of this Dog's Muscles were convulsed by this nitrated Water, as they constantly were, when fair Water passed into the Arteries. All the Blood of this Dog was very florid as it flowed out at the jugular Veins.

15. When a Gallon of Water was injected into a Dog's Arteries, in which Water two Ounces of Sal Armoniac was dissolved, all Parts were also very red. But I neglected to observe whether his Muscles were convulsed or no.

EXPERIMENT XXII.

1. **A**fter having seen many Proofs of the great Force of the Blood in the Arteries, and also in the Veins when an Animal exerts

exerts its Strength ; I thought it might not be a useless Enquiry to examine the Strength of the Coats of those Vessels.

2. I poured into an inverted glass Syphon some Mercury, so as to have the shortest Leg, which was hermetically sealed, filled within four Inches of the Top : To the other open end of the Syphon I fixed by means of a brass Pipe, one end of the right carotid Artery of a small Spaniel Dog, and to the other end of the Artery was fixed a condensing Syringe. Then placing the Artery in Water, to see whether it leaked, I impelled Air in to such a Degree, as made the Mercury compress the Air between it and the sealed Top of the Tube into so small a Compass, as shewed by Estimation, the Force to be equal to a Column of Water a hundred and ninety Feet high, or equal to the Weight of 5.42 Atmospheres ; with this Force the Artery burst at once, but no Air passed thro' its Coats before it burst.

3. The Diameter of this Artery being 0.1 Inch, the Circumference is 0.314, the whole Surface of an Inch Length of the Artery will be 0.314 of a square Inch : Now a Column of Water whose Base is a square Inch, and its Height a hundred and ninety Feet weighing

81.9 Pounds, 0.314 part of that is 25.71 Pounds, equal to the Force which an Inch Length of the Artery sustained, when it burst. And its Diameter being 0.1 Inch, the one tenth part of 81.9 Pounds or 8.19 Pounds is the Force which was requisite to burst asunder the Fibres in an Inch Length of the greatest Section of that Artery. The Force of the arterial Blood in a Dog, impelling it but eighty Inches high in a Tube; this is but $\frac{1}{26.7}$ th Part of the utmost Strength of the Artery, an Allowance being here made for the Difference between the specific Gravity of Blood and Water.

4. When the Velocities of the Pulses are increased by the brisk Motion of the Body, as in a Man from seventy five to a hundred and twenty, so in a Dog from ninety seven to a hundred and forty two in a Minute, the Quantities thrown out of the left Ventricle of the Heart, cannot be proportionably increased; for the left Ventricle cannot receive and throw out as much Blood in each very accelerated Pulse, as in the natural slower ones: Besides as the Force of the Blood increases in the Arteries, so will it on that Account pass off the faster, as also on account of the

extream capillary Vessels being enlarged, both by the greater Force and Heat of the Blood. It may therefore be reasonably concluded, that in the greatest Motion of the Body, and quickest and fullest Pulse, the Force of the Blood will not rise proportionably from eighty to one hundred and seventeen Inches; but may reasonably be supposed not to exceed a hundred Inches; that is but $\frac{1}{21.5}$ th Part of the Force which burst this Artery.

5. The carotid Artery of the Mare used in *Experiment* III. was so strong that I could not break it with my Condenser.

6. Her jugular Vein burst with a Force equal to a Column of seventy six Feet Height of Water; but it was in a part where by often bleeding it was near double its natural Diameter. The other Part of that Vein which was $\frac{1}{2}$ Inch Diameter, burst with a Force equal to a Column of Water one hundred and forty four Feet high. A jugular Vein of another Horse bore a Pressure equal to a hundred and forty eight Feet Height of Water and did not burst, but had some small Leakages, out of which a little Air escaping prevented its total bursting.

7. This

7. This Vein being 0.5 Inch Diameter, an Inch Length of its whole Surface will be 1.57 square Inch ; and a Column of Water whose Base is a square Inch, and its Height a hundred and forty eight Feet will weigh 62.9 Pounds, which multiplied into the Area of an Inch Length of the Vein gives 97.75 Pounds, the Weight which that Inch Length of Vein sustained without bursting. And the Area of its greatest Section Lengthwise being 0.5 square Inch, the Fibres in that Section sustained 31.45 Pounds without breaking.

8. Now suppose the ordinary Force of the Blood in the jugular Vein of a Horse to be equal to twelve Inches Height of Blood, this will be but $\frac{1}{13.72}$ of the Force which it sustained without bursting. And since when the Horse strained to get loose the Blood rose 52 Inches high in the Tube fixed to his jugular Vein, and would have risen somewhat higher if the Tube had been longer, suppose to sixty Inches, this will be but $\frac{1}{27.4}$ th Part of what the Vein was able to sustain.

9. I impelled Air into a Piece of the right jugular Vein of a Dog, to such a Degree as to compress the Air in the mercurial Gage, with a Force equal to the Weight of five Atmospheres,

spheres, or to a Column of Water five times thirty five Feet high, that is, a Column a hundred and seventy five Feet high, yet this Force did not break the Vein, but one of the Ligatures failed.

10. The Diameter of the Vein being 0.25, the Surface of an Inch Length of it will be 0.785 square Inch, this multiplied by 76.1 Pounds the Weight of a Column of Water whose Base is a square Inch, and its Height a hundred and seventy five Feet, gives 59.7 Pounds, the Weight which pressed on an Inch Length of this Vein; whose Diameter being 0.25 the Area of its greatest longitudinal Section is 0.25, square Inch, which multiplied into 59.7, gives 14.9 Pounds, the Weight which the Fibres in that Section sustained.

11. The common equable Force of the Blood in the jugular Veins of a Dog, being about five Inches, this is but $\frac{1}{39}$ th Part of the Force which this Vein sustained without bursting. And if we set the Force of the Blood in straining, to be twenty four, sufficient to raise it twenty four Inches in a Tube, which was the Height to which it rose, when the old Dog, *Numb.* 10. strained, see the Table, *p.* 42. then this Force will be $\frac{1}{8}$ d Part of

of the Force which this Vein sustained: An Allowance in both these Cases is made for the different specific Gravity of the Blood and Water. As there is doubtless a very great Difference in the Strength of the Fibres of young and old Animals, I had intended to have enquired in the same manner, into the comparative Strengths, not only of their Arteries and Veins, but also of their Guts. And if the Strength of the Periosteum and Ligaments of the Joynts were tried in the same manner, as in *Numb. 29.* of this *Experiment*, both on old and young Animals, there would be found a considerable Difference.

12. We see in these Instances the great Strength of the Coats of these Vessels; what great Reason have we therefore with thankful Hearts to say to our Creator, as holy *Job* did when he contemplated on the wonderful Frame and Strength of his Body, *Job. x. 11. Thou hast not only fenced me with Bones and Sinews*, but hast also effectually secured the vital Fluid, in such strongly wrought Channels, as are Proof against its most lively and vigorous Sallies, when either agitated by the different Passions, or by strong or brisk Actions of the Body.

13. It was computed above, that the Force of the Blood in the Dog's Artery could not probably increase more in its most violent Motion than from eighty its natural State to a hundred; that is one fifth part more. But in the Veins the Difference is manifestly greater; for in the Mare it rose from twelve to fifty two Inches, and would probably have risen to sixty Inches if the Tube had been longer, *i. e.* five times its natural equable Height. And in the Dog, *Numb.* 10. it rose from five to twenty four Inches, that is, near five times as high.

14. Now it was observed in *Experiment VII. Numb.* 4. that when a Dog's Belly was compressed with the Hand, the Blood would constantly rise a few Inches in the Tube fixed to his Artery, and subside again when the Hand was taken off; and in like manner the increased Force of the Blood in the Veins, seems to arise chiefly from the Constriction of the *Abdomen*: For when we exert our Strength in lifting or the like, we constantly contract the *Abdomen* what we are able, by the joint Action of all its surrounding Muscles; and the Diaphragm is also further assisted in that Action, by keeping the Lungs and Thorax distended with Wind, which is there pent in by

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hindering its Expiration at the Mouth and Nose : And as we cannot thus hold our Breath long, so neither can we thus exert our utmost Strength long, without some short Intervals of Abatement. And during this straining we constantly see the Veins of our Neck, Forehead and Temples much distended ; the Blood being forcibly impelled into them from the much contracted *Abdomen*, in whose large Veins there is Plenty of it in store ; for the Number and Capacity of all the Veins of the Body is observed to be much greater than that of the Arteries.

15. And while the Blood is thus strongly compressed in the Veins, it must proportionably retard its Passage out of the Arteries, it being therefore there accumulated, will acquire an additional Force of four Feet perpendicular Height, that is in all equal to $13\frac{1}{2}$ Feet in the straining Mare : And an additional Force of twenty four Inches, equal in all to eight Feet and eight Inches in the straining Dog ; whereby the Blood being impelled more forcibly into the Muscles enables them to act the more vigorously.

16. But the free natural Course of the arterial Blood into the Veins is not only retarded
by

by the great Compressure of the *Abdomen* on its Veins, but also when those Veins (which are the great Store-rooms of both the necessary and too redundant Fluids) are too plethoric, in proportion to the Degrees of which, the free Course of the arterial Blood will be rebated: And the Heart being in this case like a Water-Mill that is surcharged with a Flood both before and behind, its Force must needs abate and grow languid; in which case Evacuation by bleeding, is well known to restore it to its natural Vigour.

17. When the Course of the Blood is retarded by some Defect in one part, it occasions thereby a greater Flow of it on other Parts: Thus some of those that have a Limb cut off, have been observed to be subject to Hæmorrhages; and for the same Reason a scirrhous Liver and Spleen will cause a vomiting of Blood, hence also when the Spleen is cut out, the Liver increases.

18. As the Vigour of the Blood in the Arteries and Veins is thus greatly increased by the increased Pressure on the Blood-vessels in the *Abdomen*; so is its Vigour much rebated when on the contrary the Blood-vessels of the *Abdomen* are too little compressed; thus when in

a Dropſy a great deal of Water is at once drawn off by Tapping, the Patient is then in danger of dying in the Operation ; for which reaſon, when the Quantity of Water is large, it is drawn off not at once but at ſeveral times, that in the Intervals the dilated Parts of the *Abdomen* may have time to contract, and thereby duly compreſs the Blood-veſſels ; which are there made large and capacious, that they may be fit to receive the greater Stores, with which they are replenished ſome-time after Meals.

19. And thus alſo the ſmall Evacuation of a Clyſter will ſometimes cauſe a ſick Perſon to faint, or to be ready to faint ; an Argument that the vital Force of the Blood does then run low.

20. Thus alſo a natural Loofeneſs or a Purge do very ſenſibly rebate the Vigour of the Blood, by too much evacuating the *Abdomen*, as well as by the extraordinary Quantity of Fluid, that is in thoſe Caſes diſcharged from the Blood into the inteſtinal Tube. In which greatly rebated State of the Blood, the Surface of the Body being thereby reduced from a vigorously perſpiring, even ſometimes to an imbibing State, we are thereby the more ſubject to catch cold.

21. When

21. When the Vessels are in some Degree emptied by bleeding, there being then a less Quantity of Blood in the Arteries and Veins, and consequently a less Quantity passing in equal times thro' the Heart, it finds proportionably less Resistance from the arterial Blood, whence an Abatement in the Vigour of the Pulse. It being also thereby less forcibly propelled thro' the capillary Vessels, and consequently undergoing there much less Friction it grows sensibly cooler.

22. When a Vein is opened the Blood flows for that Time, not only the faster in that Vein, but also in its corresponding Arteries ; whence it is found by Experience that bleeding near the Part affected is in some cases most beneficial ; because the fine capillary Vessels of that part, in which the regular Course of the Blood is retarded, will by this means be sooner drained, than if the Bleeding were at a more remote Part ; especially at the beginning of the Disease, before the Obstruction be too firmly fixed, when bleeding near the affected Part may rather increase than abate the Inflammation. As bleeding or not bleeding in due time and Proportion are frequently of the utmost Consequence to the Patient, so it requires the

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Judgment

Judgment of the skilful Physician to know when and in what Proportion to abate by this Evacuation the Force of the Blood. And in general since a human Body is so curiously complicated an Engine, that the mutual Harmony of such innumerable Circumstances must concur to its Welfare, no wonder that it requires the masterly Hand of the skilful Physician to rectify it when out of order. Were the bold Empiric but duly sensible of this, he would not surely dare to intermeddle, but Ignorance is his best Plea.

23. Tho' the Strength of the Coats of the finer Arteries and Veins are proportionably thinner and weaker as they grow less and less; yet since the Sum of the Pressure of equally high Columns of Water against the Sides of containing cylindrical Tubes of different Diameters is proportionable to the Surfaces of those Tubes; the Strength therefore of their Coats may be equally as much superior to the utmost Efforts of the Blood in them, as those of the larger ones are to the Force of their contained Blood; they are therefore proportional to their Circumferences.

24. But as to the finer secretory Vessels and Lymphatics which arise from the capillary Arteries,

Arteries, and are out of the Course of the circulating Blood : These as they do not sustain the full *Impetus* of the arterial Blood, but only serve more slowly to seern and convey the more diluted Part of the Blood, which is proper either for Nutrition, Perspiration, &c. so neither are their Coats near so strong as those of the Arteries and Veins ; as is manifest from *Experiment XIV. Numb. 6.* where when Water flowed into the Arteries, with a Force no greater than that of the arterial Blood ; yet it passing freely by reason of its great Diluteness, into those secretory Vessels did thereby much dilate all the Parts of the Body. And it is observed that as the elastic Power of those Vessels, and also the Force of the Liquor that flows in them is but small, so are Obstructions aptest to be in the Glands, where these Vessels are both very complicated and numerous.

25. Tho' in a living Animal there is very rarely if ever, such an universal Inundation and Dilatation of all the Parts, as is here made by these Experiments on Dogs ; yet it frequently happens, that on the Stoppage of one of the natural Evacuations thro' these excretory Ducts, that others will be increased and *vice versa*.

Thus when there is too plentiful a Discharge into the Cavity of the Guts in a great Diarrhœa, the Perspiration is then small, and there is little *Saliva*. On the contrary in the Quinze the *Saliva* abounds, because the Course of the Blood is there obstructed: As also in the small Pox the Quantity of *Saliva* is large, because the Perspiration is stopped; which Stoppage of Perspiration often also increases the Quantity of the *Mucus* separated in the Nose. And from the Stoppage of Perspiration, rheumatic Pains are caused by too great Discharges of these Secretions among the Fibres of the Muscles, in the same manner as was manifestly to be seen in the Muscles of these Dogs.

26. It has been observed, on drinking a great Quantity of Water as three or four Pints at a Time, that all Parts of the Body and even the Fingers have been dilated: In this case doubtless part of this great Quantity of Water, being not thoroughly incorporated with the Blood, passes thus freely into the secretory Arteries and Lymphatics: The drinking of Waters should seem therefore to be of use in many kinds of Obstructions in these Vessels.

27. Since the Coats of these Vessels are found to be so much weaker than those of the

Arteries, from which they branch off, and since the capillary Arteries are strong enough to sustain the Impetus of the Blood in violent Exercises without swelling; it may therefore reasonably be concluded, that Inflammatory and other Swellings are often owing to the Serum's or Lymph's being, on a Stoppage in the capillary Arteries, impelled with greater Force into these secretory Vessels, which easily dilating at each Pulse, occasions the Throbbings of such Swellings: Which Swellings, compressing the Blood-vessels as was observed in *Exper. XVI. Numb. 3.* the Blood must therefore necessarily pass the adjoining capillary Arteries with greater Difficulty, whence there must needs be a greater Friction and Heat; and this inflammatory Heat may arise to a great Degree, if the mucose Coat of the Vessels is thereby rubbed off, when the Globules themselves may also be stripped of their oily Covering. Hence also on catching cold these secretory Vessels being too full for want of a due Perspiration, may by compressing the capillary Arteries, cause a feverish Heat, as well as by disordering the *Crafsis* of the Blood. Anatomists have found on tying the jugular Veins of a live Dog, and thereby greatly

greatly increasing the Force of the Blood in the Head, that the Head has swell'd : As also by tying the *Vena Cava* the *Abdomen* was fill'd with Water. Thus also when the Texture of the Blood Globules is dissolved to such a Degree as easily to enter these finer secretory Ducts, then malignant Inflammations are produced.

28. To these Experiments on the Strength of the Coats of the Blood-vessels, it may not be improper here to subjoin some that I made ten Years since to shew the Strength of the *Periosteum* and *Ligaments* of the Joints, viz.

29. I took the Instep Bone of the hinder Leg of a Calf, which was nine Weeks old, which Bone reaches from the Hock down to the pastern Joint, to which it is joined by a Joining called *Ginglimus*. I cut off from the Bone all the *Tendons*, *Ligaments* and *Periosteum*, and bored a Hole with a Gimblet, thro' the Head of the lower *Ginglimus* End of the Bone ; thro' which Hole I thrust an Iron Pin, which hindred the Noose of a Cord that was fastened to it, from slipping off the Head of the Bone. I fastened the other end of the Bone with a strong Cord to the Threshold of a Door Case : and then put the End
of

of a long Iron Bar thro' a Noose of the Rope which was fastened to the *Ginglimus* End of the Bone. The Bar rested on a *Fulcrum*, so as to become a Lever : Then I hung on the other end of the Lever Weights sufficient to draw off the Head of the Bone at the joining where it grows in length, which is called *Symphysis* ; I found the Tenacity and Resistance of this joining, to the Weight, to be equal to one hundred and nineteen Pounds when the *Periosteum* was taken off.

30. Then I took the Instep Bone of the other Leg of the same Calf, and cut off only the Tendons, but left the *Periosteum* entire, and then fixed it in the same manner as the former to the Lever ; whereby I found the Force requisite to sever the Head of the Bone from its Shank to be equal to five hundred and fifty Pounds.

31. By the Experiment on the first of these Bones, the Tenacity of the viscous Matter, at the joining of the Head to the Shank, was equal to one hundred and nineteen Pounds, which deducted from five hundred and fifty Pounds in the second Experiment, there remains four hundred and thirty one Pounds, which gives the Strength of the *Periosteum* ;
which

which is therefore doubtless, among other uses, of great Importance in strengthening the Bones to which it most closely adheres. The Circumference of the Bone where the joining *Symphysis* was forcibly separated, was about four Inches, so that the Strength of one square Inch of the *Periosteum* is equal to the Force of one hundred Pounds Weight, which Strength is much greater than the above found Strength of the Coat of the Arteries and Veins ; the great Author of Nature ever proportioning the Strength of all the Parts of the Body to the several Offices they are to perform.

32. I forcibly pulled asunder in the same manner the Hock Joint of one of those Legs, having first taken off the Muscles and Tendons. Whereby I found the Strength of the Ligaments with which that Joint was braced, to be equal to the Force of eight hundred and thirty Pounds Weight : So great Care has Nature taken to prevent the Luxation of the Joints, and so strongly are we *fenced with Bones and Sinews*, Job x. 11.

33. As a Force equal to five hundred and fifty Pounds was found requisite to separate the abovementioned joining *Symphysis*, so in the Growth of the Bone lengthwise at that join-
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ing Nature must exert a like Power ; not that we are to suppose that the growing Fibres are forcibly stretched out at each End, in the manner they were in this Experiment : Nature in the extending a growing Fibre rather effects it by the gentle Expansions of Warmth, which exerting its Efficacy on every individual Part of each Fibre, does thereby gradually lengthen it. But yet the whole Sum of this Power must be superior to the Resistance of all the Fibres which connect this Juncture.

34. It may not be improper here to subjoin an Account of some of the like Experiments which the ingenious and learned Professor *Peter Van Musschenbroek* of *Utrecht* made on several other animal Substances, viz. “ single
“ Threads of Silk from the *Chrysalis* of a
“ Silk Worm were torn asunder, some with
“ the Weight of eighty Grains, and others
“ with intermediate Weights from eighty to
“ ninety Grains. *Introductio ad cohærentiam*
“ *corporum firmerum*, p. 520, &c.

“ 35. Fifty seven of those Threads, when
“ but little twisted were as big as a human
“ Hair, these sustained 4845 Grains, when
“ they break asunder, viz. each eighty five
“ Grains.

“ 36. Affair

“ 36. A Hair of a healthy young Man’s
 “ Head, just then pulled off, sustained 2069
 “ Grains.

“ 37. Seven of these Hairs when twisted
 “ were equal in Size to a Horse’s Hair, which
 “ being untwisted sustained 9635 Grains.

“ 38. A Horse’s Hair sustained 7970 or
 “ 7920 Grains. So this Hair which was 399
 “ times bigger than a single Silk Thread sus-
 “ tained but 7970 Grains, whereas 399 Silk
 “ Threads would sustain 33915, that is 4.2
 “ times more.

“ 38. A Spider’s Thread, sixteen of which
 “ was equal to a human Hair, sustained a
 “ hundred and fifty Grains, and sixteen of
 “ them 2400 Grains.

“ 40. Twenty three Threads of the Fi-
 “ bres of Flax, which all together were equal
 “ to the Hair of a Horse, sustained 11710
 “ Grains; each of these Threads, when view-
 “ ed with a Microscope, might be seen to be
 “ composed of at least fourteen more.

“ 41. Now supposing all these abovementi-
 “ oned Threads to be of the Size of a Horse’s
 “ Hair; then the Number of Grains which
 “ each can sustain will be as follows, *viz.*

"	Silk	Spider's	Flax	Human	Horse
		Thread		Hair	Hair

" 33915. 15800. 11710. 9635. 7970.

" Whence he collects, that the finer the component Threads are, the stronger they are.

" 42. He found also that a Cord of a musical Instrument which is made of Animals Guts, sustained thrice as much as a Horse Hair of equal Size.

" 43. A Thong of Leather of a Calf's Skin which was 0.4 twelfths of an Inch broad, and 0.08 thick, scarce sustained eighty Pounds.

" 44. A Thong of Leather of an Ox's Hide which was 0.4 twelfths of an Inch broad, and 0.18 thick, sustained three hundred and eighty Pounds, whence he takes occasion to estimate the Strength of the leathern Braces by which a Coach is suspended, viz. for since such a Thong of four Lines or $\frac{1}{3}$ d of an Inch Breadth will sustain three hundred and eighty Pounds, one of three Inches Breadth will sustain 3420 Pounds, and five such sewed together to make a Brace may sustain a Weight equal to 17100 Pounds; and four such Braces

" doubled,

“ doubled, as they are in Coaches, will bear
 “ 68400 Pounds, *Amsterdam* Weight, which
 “ being to *English* Avoir-dupois, as ninety three
 “ to a hundred, will be 63611 Pounds *Eng-*
 “ *lish*. The Inch also here used by him is
 “ the *Rynland* Inch, which is to the *Eng-*
 “ *lish* Inch as 0.752 to one.

“ 55. He plainly proves also by many Ex-
 “ periments, that a twisted Rope cannot sus-
 “ tain near so great a Weight as all its single
 “ untwisted Threads can do. And that a
 “ Rope which is breaded like Womens Hair,
 “ is stronger than one twisted in the com-
 “ mon Way, in the Proportion of one hundred
 “ and seventy to twenty: Discoveries which
 “ may be of great Use in many Instances.

56. It is well known that a twisted Rope
 or Cord does contract or shorten on being
 wetted; the imbib'd Moisture dilating laterally
 their twisted Threads, whence the Cord must
 contract longitudinally; but on the contrary
 single untwisted hempen or flaxen Fibres, re-
 lax on being wetted, as animal Fibres do, tho'
 not so much.

EXPERIMENT XXIII.

1. **I** Made hydraulic and hydrostatical Experiments not only on the Arteries and Veins, but also on the intestinal Tube; by affixing in like manner Tubes of different Heights to each end of them, while they were warm.

2. I fixed a Tube to the Gullet of a Dog, and then poured in Water till the Stomach was so full, that the Water in the Tube stood thirty six Inches perpendicular Height above the Stomach; which Force burst it lengthways in its upper part near the *Pilorus*, where it was but $7 + \frac{1}{2}$ Inches in Circumference: Yet no Water was impelled thro' the *Pilorus* with this Force, tho' in some other like Experiments it has run thro' there into the Guts. Another Dog's Stomach burst in the larger left part of it, when the Height of the Column of Water in the Tube was but thirty Inches.

3. On measuring the distended Stomach of another Dog, I found its whole Surface equal to eighty square Inches, which multiplied into thirty six, the Height of the Water in the

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Tube

Tube, gives 2880 cubic Inches of Water or 104 Pounds Weight of Water, which pressed against all Sides of the Stomach: And allowing the Area of the greatest transverse Section of the Stomach to be equal to thirty square Inches, then the Pressure of the Water against the Fibres of the Stomach in that Section, when it burst, will be thirty nine Pounds. Which shews how greatly *Borelli* and *Pitcairn* were mistaken, when they estimated the Force of the Fibres of the Stomach to be equal to 12951 Pounds; since we may with good Reason conclude that the Force of those Fibres cannot in a live Animal be greater than the Force which will tear them asunder as soon as dead. Neither can the Pressure of the *Diaphragm* and of the Muscles of the *Abdomen* on the Stomach, be in our utmost Strainings greater than a Weight of Mercury two Inches deep, and of the Breadth of all their Areas, as I have shewn under *Experiment CXVI. Vol. I. p. 270.* And that the Sum of the Compressure of the Muscles of the *Abdomen* and *Diaphragm*, and also of the Stomach on its Contents, is not nearly equal to the Weight of two Inches Depth of Mercury, is evident from Appendix, *Experiment*

VII. of this 2d Vol. where it was found by a mercurial Gage fixed to the Nose of a large Pair of Smith's Bellows, that the most forcible blast of them will scarcely raise Mercury two Inches high in the Gage: And since such a blast of Wind is manifestly much greater than the most forcible puff of Wind, which is belched out of a much distended Stomach; 'tis hence evident that the Stomach even in that much distended Case does not compress what is contained in it with near that Force.

4. If we suppose the Surface of a full Stomach to be equal to eighty square Inches; and that its Contents are compressed by the Action of it, together with that of the *Diaphragm* and Muscles of the *Abdomen*, with a Force equal to one Inch depth, then the whole Pressure on its Contents will be equal to thirty nine Pounds, which is nearly the Weight of eighty cubic Inches of Mercury: But as this seems to be too great a Force by comparing the Velocity with which Wind rushes out of Bellows, when its Force is sufficient to raise Mercury an Inch in the Gage, so I believe half that Force, *viz.* about twenty Pounds, would come nearer to the Pressure on the Aliments of a full Stomach.

5. Now so small a Compressure can have very little Effect in promoting the Digestion of the Aliments: Which is therefore with good Reason principally attributed to the Concurrence of several other Causes; such as Mastication and Comminution with the Teeth, and Mixture first with the *Saliva* (which is a Leven full of elastic Air) and afterwards with the Fluid which is in plenty separated from the Glands of the Stomach; and the active and expansive Principles of this thus macerated and moistened Mass, are by the Warmth of the Stomach disposed to some Degree of Ferment: Its Solution being also at the same time greatly promoted, not only by the peristaltic muscular Action of the Stomach, whereby, as also by the Help of the prominent *Rugæ*, or Foldings of the inner Coat of the Stomach, the Mass is more intimately mixed, and so better disposed to farther and farther Solution; but also principally by the incessant reciprocal Action and Reaction of the *Diaphragm* and Muscles of the *Abdomen*, which are each about twelve hundred in an Hour.

6. And the great Use of the thus Kneading the Contents of the Stomach to and fro,
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in order to promote Digestion, is seen in the following Effect of it, *viz.* If in the Night the Stomach be loaded with a *Crapula* of indigested Food that offends it, it has been found by repeated Experience, that if the Person labouring under this Uneasiness, will for a considerable time draw in his Breath deep, almost to the Degree of sighing, the Kneadings of the Stomach being by these deep Depressions of the *Diaphragm*, double in force to what they usually are, the Stomach will by this means be much sooner exonerated of its troublesome Burthen.

7. When the Arteries of the Stomach of a Dog have been injected with Vermilion, and that Stomach has been blown up, in order to its being well dried ; in this case the Water, which had as mentioned in *Experiment IX.* flowed thro' the Arteries and Veins in order to wash out the Blood, this Water would now flow plentifully from the Veins which were not injected with Vermilion ; an Argument that there is less Blood in the Blood-vessels of the Stomach, when it is filled with Food than when empty. Whence as well as on other Accounts, the Stomach which is too much distended with Food, having then a less Quan-

tity of Blood flowing thro' its Vessels, must not only be cooler, by reason of a more retarded Motion of the Blood, which we find in some Degree after ordinary Meals, but must also have much less Moisture separated from its Glands, at a time when the too great Mass of Food requires an extraordinary Supply duly to be moistened; the Consequence of which must be a more difficult and unkindly Digestion.

8. Since there is more Blood in the Vessels of the Stomach when it is empty than when full, this greater Flow of Blood to the empty Stomach may probably contribute to increase the Appetite of Hunger; hence also it may be that the Digestion is stronger in Winter than in Summer, because less being perspired in Winter, the Quantity of Fluid retained in the Blood-vessels being thereby greater, the Blood will be more vigorously impelled, as into all other Parts of the Body, so into the Vessels of the Stomach, whence a greater Warmth, and also a more plentiful Secretion from the Glands of the Stomach, to promote the Digestion of the Aliment. The Increase also of the Appetite which is observed in the beginning of Pleurifies, is attributed to the greater Flow of
2 Blood

Blood to the Stomach, on its Passage being retarded in the *Pleura*.

9. I found the Gullet very dilatable with a small Force of Water or Wind, and hence it is that when flatulent Wind is ascending thro' it from the Stomach, the Gullet being thereby dilated, compresses the descending *Aorta*, where the Gullet is confined in its Passage between that and the Heart ; and thereby for that instant the Blood is driven more forcibly up into the Head, which often causes a short Swimming or *Vertigo* ; an Inconvenience which the Flatulent are too frequently sensible of.

EXPERIMENT XXIV.

1. **H**AVING as soon as a Dog was dead cut afunder the *Duodenum* Gut just below the *Pilorus*, I poured warm Water into the *Duodenum*, thro' a glass Tube which was fixed to it ; when the Water stood at two Feet perpendicular Height in the Tube, that Force impelled Water thro' the whole Length of the Guts, so as to flow out at the *Anus* : the *Fæces* in the *Rectum* made little Resistance, being soft and not figurated.

2. But when in another Dog a Tube was fixed to the Gullet, and Water was poured in so as to burst the Stomach and one of the Guts, yet there being hard *Fæces* in the *Rectum*, no Water passed thro' it.

3. From this Experiment we see how requisite it is, in some colicky Obstructions of the Bowels, to promote the Operation of Purgatives with Clysters; without which, Purgatives may in some Cases do more harm than good, while they increase the painful Distention of the Bowels, without being able to pass and carry off the noxious Mass.

4. I am very sensible that there was wanting in this Experiment, the peristaltic Motion of the Guts, which in a live Animal promotes the Descent of their Contents. But 'tis to be feared that when in the *Iliack Passion*, there is a Stoppage in any part of the Guts, either by *Fæces* or distending Wind, if that Distention be superior to the Force of the arterial Blood, it must necessarily stop the Course of the Blood in that part; which must consequently both retard the peristaltic Motion, and also cause an Inflammation; which if not soon prevented too often ends in a Mortification.

EXPERIMENT XXV.

1. **I** Cut afunder the *Rectum* of a Dog, and poured in warm Water thro' a Tube fixed to it; the Water gradually passed the Valve at the *Cæcum*, and flowed along the Guts, up to the *Pylorus*: The perpendicular Height of the Water in the Tube was five Feet.

2. In another Dog, I found the Valve at the *Cæcum* shut so close that neither Water nor Air would pass it, not even when cleansed from the *Fæces*.

3. I fixed also a Tube to the *Rectum* of a third Dog, and poured in warm Water, till it stood twenty Inches high in the Tube; yet it did not pass above six or eight Inches up the *Rectum* being hindered by the *Fæces*, which yet were not hard enough to be figurated: But when the *Fæces* were removed, then Water freely passed the annular Valve at the *Cæcum*. Hence we see how requisite it is in some Cases, first to get the *Fæces* below the Valve out by a Clyster; after which the Effects of a second Clyster may reach much farther.

4. *Qu.* Have we not from hence as also from Numb. 1. of this Experiment, a hint
to

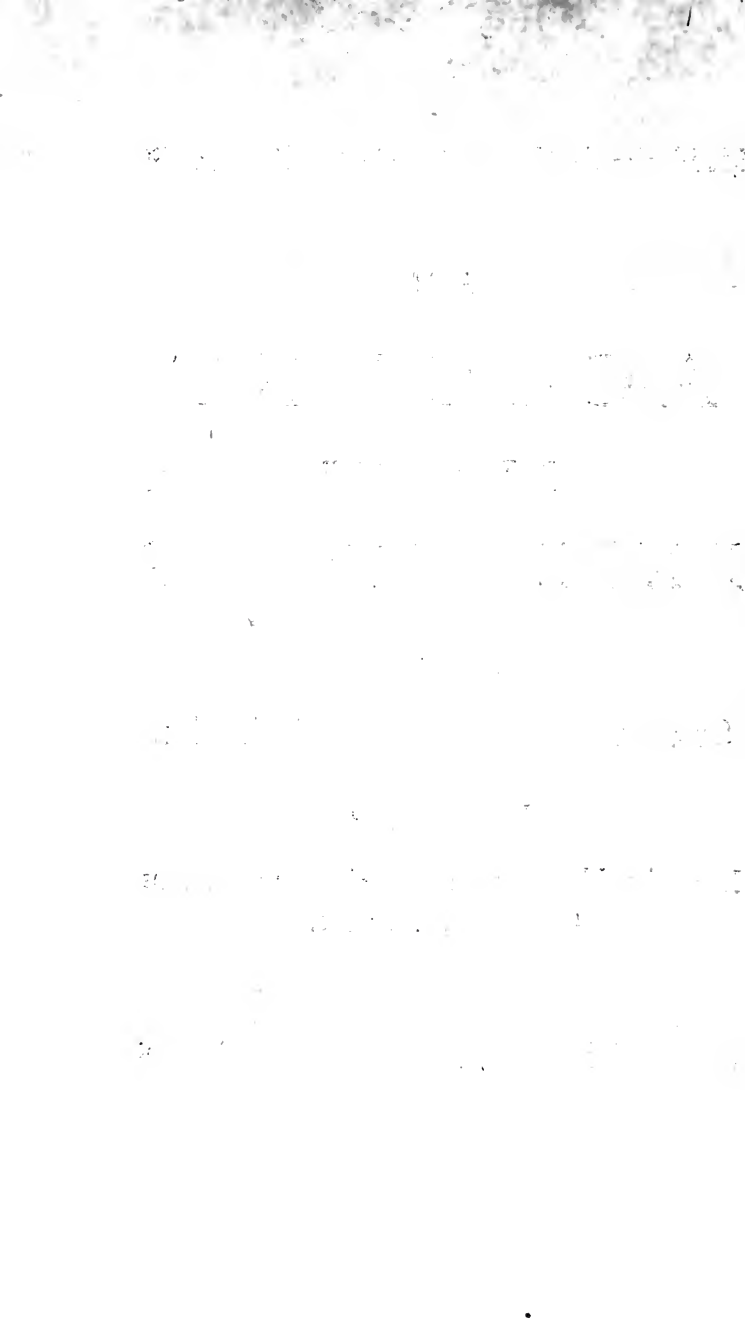
to try, at least in some desperate cases, as in the *Iliack Passion*, whether any Relief might be had by applying a Clyster thro' such a perpendicular Height as shall be judged proper; this might probably pass up to the affected part, and so possibly not only open a free Passage, but also by its proper Vertue, might allswage an Inflammation, and thereby prevent a Mortification. But the weaker the Patient is, of the less perpendicular Height ought the Tube to be, thro' which the Clyster passes; else its Force may be too great for that of the arterial Blood; and so either by stopping, or at least too much retarding its Motion thro' the Coats of the Guts, may endanger Life.

5. If Clysters of different Qualities were thus injected into live Dogs from different perpendicular Heights, a more certain Judgment might be made, how far they might be used with Safety, and also whether any Benefit may reasonably be expected from them.



A N
A C C O U N T
O F S O M E
E X P E R I M E N T S
O N
S T O N E S in the Kidnies and Bladder.
With an Inquiry
Into the N A T U R E of those anomalous
C O N C R E T I O N S.





THE INTRODUCTION.

I. **I** Am sensible that some may be apt to think, it savours too much of Vanity and Presumption, for any one, after such innumerable fruitless Researches of the ablest Chymists, to attempt to find out a safe Dissolvent of the Stone in the *Bladder*. The great Tenderneſs of thoſe Parts on the one hand, and the extream Hardneſs of many of the *Calculi* on the other, do with too much Probability, make the caſe ſeem deſperate. For after applying all imaginable chymical Preparations, it is found, that neither the alkaline, nor acid, the neutral, ſaline, ſulphureous, nor ſaponaceous Menſtruums, have any Effect on theſe Concretions; Spirit of Nitre, which is too corroſive, being the only thing which has hitherto been found effectually to diſſolve them. Yet in a Matter of ſo great Importance, to the Eaſe and Welfare of a conſiderable Part of Mankind, we ought not wholly

to despair of finding some proper and safe Dissolvent. And we are hereto excited by the greatest and most knowing Chymists, who do frequently encourage us, not to give over this Research; being sensible there were Menstruums powerful enough to dissolve Metals and Stones, yet so mild as not to hurt, or offend the tenderest part of the Body.

2. What more particularly induced me to make some few Attempts in this Matter, was, that when I was endeavouring some time since to analyse the Air by many *Chymio-Statical* Experiments, among other Trials, I found that by Distillation, vegetable Tartar, viz. that of Wine, contained above five hundred times its Bulk of Air, which was a great deal more than I found, bulk for bulk, in any the fluid or solid Parts of Vegetables. This put me upon trying whether it was the same with animal Tartar, viz. *The calculus humanus*.

3. I was furnished with Variety of *Calculi* by the Favour of Mr. *Ranby*, Surgeon to his Majesty's Household, F. R. S.

EXPERIMENT I.

I. **A** Piece of *Calculus* that weighed two hundred and thirty Grains, and was something less than $\frac{1}{4}$ of a cubic Inch in bulk, being distilled in a Gun Barrel, as describ'd in *Vegetable Staticks*, Vol. I. *Exper.* LXXVII. p. 189. there arose from it, in Distillation five hundred and sixteen cubic Inches of elastic Air, that is a Bulk equal to six hundred and forty five times the Bulk of the Stone, so that above half the Stone was rais'd by the Action of Fire, into elastic Air. The remaining *Calx* weighed forty nine Grains, that is $\frac{1}{4.69}$ part of the *Calculus*; which is nearly the same Proportion of *Calx*, that Dr. *Slare* found remaining, after the Distillation, and calcining two Ounces of *Calculus*, " one Ounce and three " Drams of which, he says, evaporated in " the open Fire (a material Circumstance, " which the Chymists rarely enquire after) " of which we have no Account." *Philos. Transact. Lowthorp's Abridgment*, Vol. III. p. 179. the greatest part of which was, we see by this Experiment, rais'd into permanently elastic Air.

2. Hence

2. Hence it was found that the *Calculus* and Tartar of Wine did yield more Air in Distillation, than any other Substances, whether animal, vegetable or mineral. And it was remarkable that a greater Proportion of this new raised Air from these two Substances, was re-forbed and lost its Elasticity, in standing a few Days, than that of any other Parts of Animals or Vegetables, which are strong Symptoms that the *Calculus* is a true animal Tartar. And as there was very considerably less Oil, in the Distillation of Rhenish Tartars, than there was in the Distillation of the Seeds and solid Parts of Vegetables; so I found that this *Calculus* contained much less Oil, than the Blood or solid Parts of Animals. But some Stones which were taken out of a human Gall-bladder yielded in Distillation more Oil, and much Air.

3. A small Stone of the Gall-bladder, which was of the Size of a Pea, was dissolved in a *Lixivium* of *Sal Tartar* in seven Days, which *Lixivium* will also dissolve Tartar, yet it will not dissolve the *Calculus*, tho' I boiled a hard Piece in it for several Hours.

4. A Quantity of *Calculus* equal to one half of what was distilled, *viz.* a hundred

and fifteen Grains, did, when a cubic Inch of Sp. of Nitre was poured on it, dissolve in two or three Hours, with a large Froth, and generated forty eight cubic Inches of Air; none of which lost its Elasticity, in standing many Days. A like Quantity of Tartar, having Spirit of Nitre poured on it, was at the same time dissolved, but no elastic Air was generated, notwithstanding Tartar abounds so much with Air.

5. Small Pieces of Tartar and *Calculus* were in twelve or fourteen Days dissolved in Oil of Vitriol; the like Pieces of Tartar and *Calculus* were dissolved in a few Hours by Oil of Vitriol, into which there was gradually poured near an equal Quantity of Spirit of Harts-horn made with Lime, which Mixture made a considerable Ebullition and Heat.

6. Tho' the remaining *Calx* of the Distillation of Tartar, runs *per deliquium*, and has therefore *Sal Tartar* in it; and tho' the *Calx* of distilled *Calculus*, does not run *per deliquium*, and has consequently no *Sal Tartar* in it; yet it cannot thence be inferred, that the *Calculus* is not a tartarine Substance; because *Sal Tartar* it self, when mixed with an animal *Calx*, distills all over, so that the *Calx*

will not afterwards run *per deliquium*. By the great Similitude there is therefore in so many Respects, between these two Substances, we may well look upon the *Calculus* as a true *Animal Tartar*, and doubtless gouty Concretions are the same.

7. *Animal Tartar* I would call it in Contradistinction to *Vegetable Tartar*; for as animal and vegetable Substances greatly differ in their Salts and Sulphurs when chymically analysed, so do also their respective Tartars: But then they are found exactly to agree in these remarkable Properties, *viz.* That they are both formed not barely as a Sediment at the bottom of their Liquors; but being equally expanded and separated from all parts of their Fluids, they adhere uniformly to the Sides of their containing Vessels in the Form of a hard Crust: Those Particles which are immediately next the Sides of the Vessel being first attracted and adhering to them. And as it is a known and wonderful Property of Fluids, to diffuse what is mixed with them equally in every part, so when part of these incrusting Particles are attracted out of the Fluids; the remaining Particles equally diffusing themselves, a new Succession of them is thereby brought within the Attraction of the
already

already incrusting Particles, and so on successively : Otherwise the Particles which float in the Fluid at some Distance from the Sides of the Vessel, could not be attracted thither, for this Attraction of Cohesion acts only near. Further, these Tartars are both observed to be deposited in greatest Plenty, when the Sides of the Vessels have been already incrustated : They agree also in this that the animal and vegetable Fluids deposit their incrusting Matter, in greatest Plenty when they are in an attenuated State, and the more attenuated they are, the harder will their Incrustations be. They both agree also in this, that they yield in Distillation more Air than any animal or vegetable Substance. With good reason therefore has the *Calculus* been called a Tartar by many. And as the *Germans* call the Tartar of Wine, Wine Stone, so the *Calculus* may be called Urine Stone ; as also the Incrustations from mineral and other Waters may be called Water Stones : But as they are all formed in the same manner, and have several of the same Properties in common, so they may not improperly be called the Tartars of those several Fluids.

8. From the great Quantities of Air, that are found in these Tartars, we see that unelastic Air Particles, which by their strongly attracting Property, are so instrumental in forming the nutritive Matter of Animals and Vegetables, is by the same attractive Power, apt sometimes to form anomalous Concretions, as the Stone, &c. in Animals, especially in those Places where any animal Fluids are in a stagnant State, as in the Urine and Gall-Bladders. They strongly adhere also to the Sides of Urinals, &c. the like tartarine Concretions are also frequently formed in some Fruits, particularly in Pears; but they do then especially coalesce in greatest Plenty, when the vegetable Juices are in a stagnant State, as in Wine Vessels, &c.

9. This great Quantity of attracting, unelastic Air Particles, which we find in the *Calculus*, should rather encourage than discourage us, in searching after some proper Dissolvent of the Stone in the Bladder, which in the *Analysis* of it is found to be well stored with active Principles, such as are the great Agents in Fermentation. For Mr. *Boyle* found therein a good Quantity of volatile Salt, with some Oil, and we see by these Experiments, that
there

there is store of unelastick Air Particles in it, which are raised in Distillation into an elastic State, at the same time that the volatile Salt arises in white Fumes, an Argument that they are intimately combined with each other in the *Calculus*.

10. And it was this Consideration, which put me upon making some Attempts, to try by Variety of fermenting Mixtures, whether the brisk Action of some Fermentations, might not possibly so shake and rouse these Air Particles, as to make them fly off into an elastic State, and thereby dissolve the Union of the Parts of the Stone. We find that Spirit of Nitre acts in this manner on the *Calculus*, in the Dissolution of it; for we see in *Experiment I. Numb. 4.* that a hundred and fifteen Grains or about $\frac{1}{3}$ of a cubic Inch of *Calculus*, when dissolved in Spirit of Nitre, generated forty eight cubic Inches of Air, with a large Froth, a Quantity equal to a hundred and forty four times the Bulk of the *Calculus*.

11. From the few Tryals I have hitherto made, I have found a fermenting Mixture, which by its vigorous Action, not only throws off elastic Air from many human *Calculi*, but also very sensibly wastes and dissolves several of

those which are of a less compact Texture; especially the Gravel Stones, which are not generally so hard, as Stones which have had a longer time to harden in the Bladder; yet I have met with some of these Gravels so hard as not to be dissolved by this Mixture.

12. Tho' I have not herein succeeded so far, as to encourage any one to attempt the dissolving therewith Gravel Stones in the Bladder: Yet I thought it might not be improper to give an Account of the small Progress I had made herein; since it might possibly be a Step, towards the happy and important Discovery, by exciting and engaging others in the same Pursuit; who among the infinite Variety of Menstruums that may be compounded, may haply hit upon some, which may more easily dissolve most Gravel Stones at least, if not the larger and harder *Calculi*. If we could make no farther Advance than this, it would be of considerable Benefit to Mankind, by taking away, when it first falls into the Bladder the *Nucleus* on which larger and harder Stones are in time formed. And this would be effected, if with a few Injections of a safe and proper Menstruum, we could only dissolve a small part of a large Gravel Stone, lately
fallen

fallen from the Kidnies, so as to make it fit to pass off thro' the *Urethra*; which it would also do with more ease to the Patient, by having thereby, its Surface and sharper Points softened and made mucilaginous, and less stimulating. This is what the fermenting Mixture I am going to give an Account of, will do on some of the softer Stones, but not without too many repeated Injections, to make it practicable with Safety to the Patient.

13. That I might with the greater Accuracy and Readiness vary and adjust, as occasion required, the several Proportions of the fermenting Mixtures, I divided the Capacity of several Glass Vessels, out of which I poured the Liquors, into cubic Inches; making Marks with a File on the Outside of the Glasses, at each Division. I divided also the Capacity of a large Tube into quarter cubic Inches, it was half Inch Diameter, and sealed at one end. I made also several Divisions on a short Tube which was $\frac{1}{4}$ Inch Diameter, the Capacity of each Division containing ten Drops of Oil of Sulphur; so that by dipping one end of this Tube in Liquor to any of these Marks, and then stopping the other end with my Finger, I could readily take up 10, 20, 30, 40 or 50

O 4 Drops,

Drops, or any intermediate Number.

14. I will now give a short Account of some of the principal Experiments I made; in doing of which I did not confine myself to such mild Mixtures, as might probably not be injurious to the Bladder; but chose to begin with the strongest fermenting Mixtures, in hopes that if I should find any of these would dissolve the *Calculus*, by gradually weakening and lowering such Mixture, I might possibly bring it to such a Degree of Mildness, as might make it not injurious to the Bladder, and yet retain in some measure its dissolving Quality. Or if I should not be so happy as to succeed so far, yet I thought it most probable, that I might find Dissolvents, among these stronger Mixtures, which might at least give some farther Insight into the Nature of the *Calculus*.

EXPERIMENT II.

1. **A** Cubic Inch of Oil of Vitriol, and double its Quantity of Water, when mixt together, made so hot a Ferment that I could hardly bear to hold my Hand at the Bottom of the Bolthead; yet it had no Effect on a Piece of the hardest kind of *Calculus*. And

it was the same when the Heat and Ferment was renewed by throwing in some Filings of Iron.

2. The like Proportions of Oil Vit. and Water, with several kinds of powdered vitriolic Stones or *Pyrites*, made violent Ferments, but had no Effect on this very hard *Calculus*.

3. It was the same with Oil of Vitriol and other acid Spirits, when poured on several alkaline Bodies, such as powdered *Belemnites*, *Asteria*, Coral and Oyster-shell, &c.

4. But Oil of Vitriol mixed with unrectified Spirit of C. C. tho' it would not dissolve the abovementioned very hard Piece of *Calculus*, yet with ten or twelve fresh Mixtures, it rotted and dissolved several pretty thick *Lamina* or Cloves of other *Calculi*; which tho' not of so compact a Texture as the other, yet were so hard that I could not make any Impression on them with my Nail: But this was too pungent a Mixture to give any Hopes that the Bladder can bear any Degree of it.

5. Spirit of Rye-bread being found by Chymists a Dissolvent so powerful as to dissolve several kinds of Stones and hard Substances, and yet withal so mild, that it may with great Safety be held in the Hollow of the Hand; I prepared

prepared a good Quantity of it, both unrec-
tified and rectified, and made it one in a great
Variety of briskly fermenting Mixtures, in
hopes that by thus putting its Parts in a brisk
Motion, it might have made some Impression
on the *Calculus*, but it had no Effect.

6. I made a Preparation of Tartar of Vi-
triol, by mixing one part of Oil of Vitriol,
with twice its Quantity of hot Water, in
which were Pieces of *Calculus* and Tartar;
the *Calculus* emitted Bubbles, but not the
Tartar: Then I poured in gradually *Ol. Tart.*
when both *Calculus* and Tartar emitted for
some Minutes a great Quantity of Bubbles. The
Tartar was almost at once dissolved, and the
Calculus was very rotten and brittle, but it
was not of the very hard kind. *Sal Tartar*
which is a fixt Alkaly, being less corrosive
than Spirit of Harts-horn which is a volatile
Alkaly.

EXPERIMENT III.

1. **I** Made a Solution of it in Water, in the
Proportion of an Ounce of *Sal Tartar*
to four of Water; and made many brisk Fer-
mentations, with several Portions of this al-
kaline

kaline Mixture, by pouring in of the strongest acid Spirits, as Spirit of Nitre, Spirit of Salt, Spirit and Oil of Vitriol, and Oil of Sulphur. I found Oil of Vitriol and Oil of Sulphur most effectual to my Purpose, and of these two, I chose rather to make use of Oil of Sulphur than Oil of Vitriol, as being the purer Acid of the two, and less noxious to animal Bodies than Oil of Vitriol.

2. I found after the Mixture of a great Variety of Proportions of these Liquors, that the following were the properest for my Purpose, *viz.* one cubic Inch of Water, one third of a cubic Inch of Solution of *Sal Tartar*, and twenty five or sometimes thirty Drops of Oil of Sulphur. Also six cubic Inches of Water, $\frac{1}{4}$ Cub. of the Solution of *Sal Tartar*, and fifty Drops of Oil of Sulphur.

3. These Proportions fermented briskly, and made Air Bubbles arise very fast from the *Calculi* for eight or ten Minutes, as would also several other Proportions, tho' I did not find any so effectual as these, which after several fresh Affusions of them, would dissolve some *Calculi* which were hard to the Touch: They would also dissolve several Gravel Stones, tho' not all; nor had they any Effect on many of the hardest *Calculi*.

4. If

4. If Air do not arise briskly from the *Calculus*, upon pouring on any of these Mixtures, the Rule to adjust the Proportions accurately, is to drop in a few Drops of Oil of Sulphur; if this Addition make more Air arise from the *Calculus*, it is a sign some of it was wanting, but if more Air does not arise, then it is a sign that more Solution of *Sal Tartar* is wanting.

5. It fermented best, when half the Water was poured on the Solution of *Sal Tartar*, and the Spirit of Sulphur was dropped into the other half, and then both Mixtures poured together. Warm Water was better than cold, tho' it fermented longer cold.

6. When there was a double Quantity of Oil of Sulphur, I have not found it more powerful in dissolving, and when the Solution of *Sal Tartar* was much stronger the Ferment was less.

7. This Liquor has no Effect on the *Calculus* after the Ferment is over, as I have found by laying several *Calculi* to soak in it for a whole Year. So that the Effect it has on the *Calculus* while it is fermenting, seems not to depend on the Fitness of the Particles of the Menstruum to enter the Pores of the *Calculus*, but rather to certain harmonic Proportions

portions between the Vibrations of the fermenting Liquor, and the Tone or Degree of Tenseness of the Parts of the *Calculus*: Just as when two Strings are equally tense, the striking of one will make the other sound; or as I have often observed the different Pipes of an Organ, will make different Boards vibrate, according to the Uniformity there is, between the Tenseness of the Fibres of the several Boards, and the Tone of the different Pipes.

8. And in like manner we may not unreasonably suppose, that when there is the like Uniformity between the Vibrations of the fermenting Liquor, and those of the Parts of the *Calculus*, that the *Itus* and *Reditus* of these Vibrations, increasing in this Case each other's Force, some parts of the *Calculus* are thereby thrown off into elastic Air. In Confirmation of this Conjecture, I have observed Air to arise briskly from a *Calculus* with twenty or ten Drops of Oil of Sulphur, and yet with fifteen Drops little or no Air arose, notwithstanding the Proportion of the other Ingredients of the fermenting Mixtures, was the same in all three Cases.

E X P E R I M E N T IV.

1. **A** *Calculus* which weighed three hundred and fourteen Grains, after forty nine fresh Affusions of the abovementioned dissolving Menstruum was wasted a hundred and thirty four Grains; but the remaining *Nucleus* of this *Calculus* was so very hard, that this Liquor had no Effect on it.

2. I dissolved also several other of the softer kind of *Calculi* and the broken Shells or Cloves of others; but there were many of the harder kind, which this Liquor had no sensible Effect upon.

3. When a large *Calculus* was sawn asunder, and immersed in the fermenting Liquor, I observed the Air to rise in much greater Quantity, from the softer inward part of the *Calculus*, than from the hard polished Surface.

4. As for Gravel Stones I was obliged to several Persons who were so good as to furnish me with 'em, which I made the following Experiments upon, *viz.*

5. Some small Grains of a redish yellow gritty Gravel were crumbled into coarse Sand with seven Affusions of one cub. Water, $\frac{1}{2}$ cub.

cub. of Solution of *Sal Tartar*, and twenty five Drops of Oil of Sulphur ; the Air which rose from the Gravel, adhering to it in large Bubbles, made the Gravel frequently rise to the Top of the Liquor.

6. I put into the same Menstruum a Clove or Fragment of a Scale of a larger *Calculus*, which was voided the Day before, it was $\frac{1}{16}$ th part of an Inch thick, and $\frac{1}{8}$ broad ; of an ash Colour, so hard that it was difficult to break away its Edges with ones Nail. After thirty six Affusions of this Liquor, it was rotten as Dirt, tho' it retained its Shape.

7. Three hard redish Gravels, of a third Person's, which were as big as large Shot, were after eleven Affusions of the same Liquor, considerably more brittle, their Surface rotten, and much diminished, and after twenty six more Affusions, they were wasted to the Size of Pins-heads.

8. I layed two other Gravel Stones of the same Person's, but somewhat less than the former, to soak in Urine for twenty four Hours, and then poured on eight Affusions of the dissolving Menstruum : The following Night they were laid to soak again in Urine, and the next Day I poured on sixteen more Affusions ;

after which one was wasted and the other very rotten; so the soaking in Urine does not hinder Diffolution.

9. I dissolved in the same manner the Gravel of three other Persons, which were either of an ash Colour, or such as seemed to be composed of a coarse redish sandy Grit.

10. But the larger Gravel of two other Persons, which had probably lain much longer a hardening in the Kidnies or Bladder, and were covered with a kind of hard Polish, had very little Impression made on them by this Liquor; yet some Gritt came off the Surface of one of these after eleven Affusions; and after thirty more Affusions, each end was soft and crumbled. But this Liquor made no Impression on the Gravel of the other Person.

11. This Liquor dissolved a Piece of *Tartar* in five Affusions, but a like Piece of Tartar lay thirty Hours in the same Liquor, before it was dissolved, when there were no repeated Affusions.

12. That all the Air Bubbles which arise during the Ferment do not arise from the *Calculus*, is plain, because some arises in that part of the Glass Vessel where there is no *Calculus*, these Bubbles therefore must arise from
the

the fermenting Mixture, for *Sal Tartar* by *Experiment LXXIV. Vol. I. p. 184.* contains a good Quantity of Air. Yet far the greatest Quantity of Air arises from the *Calculus*, as is evidently to be seen, when two like Quantities of this Liquor are poured at the same time into two Glass Vessels, which have, one of them, a *Calculus* in it, and the other none.

13. Tho' this Menstruum is far from being efficacious enough to encourage any one to make the Tryal on human Bodies, it requiring as we see too many repeated Injections to dissolve some of the softer kinds of *Calculi*; yet I thought it not improper to try whether the Bladder could bear a Liquor with this Degree of Acidity in it; it being so mild that it may be taken into the Mouth without Inconvenience, tho' it sets an austere Roughness on the Teeth.

EXPERIMENT V.

1. **I**njected therefore at three several times thro' a Tunnel and Tube about half a Pint of this Liquor into the Bladder of a Dog, after which he did not shew any Signs of Uneasiness: But when another time a Pint

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and

and half of near double the Strength of the former was injected, he seemed uneasy as if troubled with the Strangury, but the uneasy Symptoms went off in half an Hour. Some Days after I killed the Dog, but could not perceive on opening him, that the injected Liquor had done any harm to his Bladder.

2. These Injections are easily made into a Dog's Bladder, by passing a Catheter directly into it, thro' an artificial *Fistula* made in the *Perinæum*, as directed by Mr. *John Douglas*, Surgeon, F.R.S. in the *Philosoph. Transf. Numb.* 399.

3. I injected also the same Liquor twelve several Days into the Bladder of a Bitch, after which she sometimes shewed some Uneasiness for a little while and sometimes not. She continued very lively and brisk long after; but some Months after in the following Summer, she was in Copulation Dog-drawn as it is called, *i. e.* the Vagina was drawn out, which caused her Death, whence we may conclude, that its Fibres were hardened and contracted by the acid Spirit of Sulphur, and it may probably have had the like Effect on the Fibres of the Bladder. It is therefore by no means adviseable

adviseable to attempt the Use of it in a human Bladder; neither do I here give an Account of it with any such View; my Design herein being only to shew, that this is at least some Advance towards discovering a safe dissolvent for most Gravels and some of the softer kinds of *Calculi*; but as to the harder kinds, on which nothing has hitherto been found to have any effect towards a Dissolution, except *Aqua-fortis*, there is little Hopes of ever finding a safe Dissolvent for them. But as to the softer kinds there are several mild acid Mixtures that will dissolve them, when laid to soak in them for a few Days.

4. I tried to make this Liquor more mild by mucilaginous Mixtures; such as Solution of Gum Arabick, and Decoction of Comfrey Root; but I did not find it signify'd any thing; it served only to increase the Froth in the Effervescence, which was also the Inconvenience of using Urine instead of Water: For I dissolved several Pieces of *Calculus*, with Urine Solution of *Sal Tartar* and Oil of Sulph. so that a little Urine in the Bladder would be no Impediment to the Operation of this Liquor.

EXPERIMENT VI.

1. **T**Hinking it might possibly be of use in Experiments of this kind, to have a continual Flow of Liquor in and out of the Bladder, I directed an ingenious Artist to make me a double *Catheter*, the Cavity of which was divided lengthwise by a thin Partition, into two separate Channels which ended in two divaricating Branches. To one of these Branches was fasten'd an Ox's Ureter, or the Windpipe of a Goose or Turkey, which by means of a Glass Tube either communicates with or has Water flowing from a capacious Vessel, placed three Feet above the *Catheter*: So the Liquor as it flows from the Vessel, runs thro' the one half of the *Catheter*, and flows into the Bladder, and then returns out of the Bladder thro' the other half of the *Catheter*.

2. Thro' this Instrument I caused twenty three cubic Inches of the dissolving Liquor to flow in and out of the abovementioned Bitch's Bladder in a continual Stream: And after that for four Hours and a half, there ran incessantly in and out in the same manner a
Stream

Stream of Water of the Warmth of Urine in quantity about nine hundred cubic Inches or three Gallons Ale Measure, and this without the least Harm or Inconvenience to the Bitch, that I could perceive; sometimes when the Bladder was pretty full, the Water flowed out between the *Sphincter* and the *Catheter*, and then ceased to flow till it had filled the *Bladder* to the same Degree of Fulness again.

3. Dr. Keill in his *Medicina Statica Britannica*, p. 14. observes that the Quantity of Urine which he made in twenty four Hours Day and Night, was about thirty nine Ounces, of which he says twenty one Ounces may be allowed for what passed off in twelve Hours Day, which is near at the rate of two Ounces in an Hour, in the Day-time; which supposing it to be at that rate, then there having passed thro' the double *Catheter*, nine hundred cubic Inches in four Hours and a half; that is at the rate of two hundred cubic Inches, or a hundred and thirteen Ounces in an Hour: Then the Proportion the Urine, which will flow into the *Bladder* in that time, will bear to the Water, which passed thro' the *Catheter* in the same time, will be as one to fifty six, which proportion may be much di-

minished, by increasing the perpendicular Height; and consequently the Force of the Water which flows thro' the *Catheter*, as also by abating on such an Occasion the Quantity of Drink that is taken in. This small Proportion of Urine to Water may probably very little disturb the Effect of any Liquor, that may by this means be made to pass into the Bladder; whether for dissolving of the *Calculus*, if any such Liquor should ever be discovered, or whether it be for any medicinal Purpose to cure any Disorders in the *Bladder*; in which cases this Instrument may possibly be sometimes of use.

4. But if upon Tryal it be found that the Canals of this double *Catheter*, are too small, then it may perhaps be a better way to pass any Liquor into the *Bladder*, thro' a small or middle sized common *Catheter*, thro' which it would flow more plentifully in, and also pass out more freely, between the *Sphincter* and the *Catheter*, when the *Bladder* is full to a certain Degree, whereby any gross, slimy or fæculent Matter would more easily be discharged from the *Bladder*, than thro' the narrower Passage of a double *Catheter*.

EXPERIMENT VII.

1. **I** Pounded severally the three following hot alkaline Plants, viz. *Onions*, *Scurvy-grass* and *Horse-radish* Roots, and put the Pulp into three Pots, in the middle of which I put very hard *Calculi* which were all taken out of the same Person. I pressed the Mash hard down, and set the Pots in a hot Bed for thirteen Days.

2. The *Scurvy-grass* and *Horse-radish* had no sensible Effect on their *Calculi*: But the Surface of that which was put into the *Onions* was so soft that I could scrape it off with my Nail; and it was the same when I put a like *Calculus* into Onion Juice mixed with Water, which was kept warm in a Chimney Corner for fifteen Days; in which Liquor the redish gritty Gravel of two other Persons was at the same time dissolved.

3. Hence *Onion* Juice seems to have some considerable Efficacy in dissolving the *Calculus*: And where frequently eaten should, if not waste, at least prevent the Increase of, the *Calculus*.

EXPERIMENT VIII.

1. **I**N the History of the *Royal Academy* of Sciences at *Paris*, An. 1720. there are Accounts of several *Calculi* being dissolved by laying them many Days in Water, sooner or later according to their different Degrees of Hardness.

2. I took some of the abovementioned, ash-coloured, and round redish Gravel Stones, and put some of them at the same time into cold and warm Water, and found those in warm Water dissolv'd much sooner than the other, they being covered with a white *Mucus*.

3. I laid several Gravel Stones in a little Rill of warm Water for fourteen Days and a good part of those Nights: They had several of them a white *Mucus* over them; but they did not dissolve so fast as those in stagnant warm Water; the Reason of which might be that this running Water was but half so warm as the abovementioned stagnant Water; and they were besides some part of each Night cold, when the Water ceased to run.

4. I poured into a *Florence Flask* thirty nine cubic Inches of Water, and one of fresh U-
rine,

rine, and put therein the Gravel of seven different Persons, and then set the Flask in hot Dung, where it had a Heat equal to that of Blood; but they had very little *Mucus* on them in six Days, so that this small Proportion of Urine seems to be some hindrance to Diffolution.

5. What I had principally in view in making these last mentioned Experiments, was to see whether we might have any reasonable Expectation of wasting in some Degree, Gravel Stones, by a continued Series of Diuretics; but they give us very little Encouragement to hope for any other Success from thence, than barely the washing down of the Gravel if not grown too large. It seems indeed probable that while Diuretics are using, the Gravel or Stone may make a very little if any Progress in increasing in bulk, because at that Time, the Urine is more diluted and less rancid and urinous; and consequently not so much charged with tartarine Particles: For it is observed that as those of a hot Temperament, and of the hardiest Constitutions, have the most rancid Urine, so they are generally more subject to the Stone than others: Not only because they are supposed to perspire the
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watry Matter more plentifully, but also because their Urine is more highly alkalized, attenuated and digested, and its tartarine Parts thereby subtilized, and rendered less mucilaginous than the Urine of those of a less robust and more lax Constitution of Fibres, which is probably a principal Reason why Women are much less subject to the Gravel or Stone than Men.

6. As Fermentation breaks and dissolves the mucilaginous Texture of vegetable Fluids, so is the like Texture of animal Fluids dissolved in proportion to the Degree of Digestion which they undergo. For as all Degrees of Ferment in vegetable Fluids have a Tendency from Viscidity to Acidity: So all Degrees of Digestion, from its first beginning in the Stomach, thro' its whole Course, have a Tendency to Putrefaction: But in a healthy State of an Animal, this Tendency is stopped at a certain Degree, by the sweet Emulsion of fresh Nutriment, without a constant Supply of which, the animal Fluids would soon tend to a fatal Degree of Putrefaction, and Putrefaction dissolves Viscidities. And accordingly it is observed that Urine deposits its Tartar more freely to the Sides of Urinals, when it has stood some time to putrifie, and to become thereby

thereby less viscid and more dilute. Thus also *Must* or the unfermented Juice of Grapes yields no Tartar; nor do Wines deposit their Tartars to the Sides of Vessels, while they are in a thick, turbid, mucilaginous State; but when by a farther Degree of Ferment, they are so much diluted as to fall fine, then the tartarine Particles being disintangled, do plentifully adhere to the Sides of Vessels, so as to form thereby a hard Crust. And as the Tartar of Wine is observed to be the harder, the finer and more attenuated the Wine is, so probably the more digested and attenuated the Urine is, the *Calculus* will be proportionably the harder; and frequently to a very great Degree of Hardness, notwithstanding it lays constantly in Urine; thus the Mortar of Walls which are so situated under ground as to be never dry, is often observed to be harder than that which lays dry; and thus the Bones of Animals, and the woody Substance of Trees harden in a continually moist State: And as Naturalists have observed that Bones of Animals are more compact and harder in hot than in cold Climates, so 'tis probable that *cæteris paribus* the *Calculus* may be harder in hot than in cool Constitutions; agreeably to what

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Dr. *Frederic Hoffman* observed of the stony Incrustations in the Caroline Bath, which are hardest and redest, but in least Quantity at the Mouth of the Spring when the Water is hottest; but as the Water grows cooler at a farther Distance from the Spring Head, there the Incrustation is greater in Quantity, but softer, more friable, and like Sand. *Disquisitio Physico-Medica de Thermis Carolinis.*

7. Nature seems to have given us a hint, that soft mucilaginous Things are good to prevent the Growth of the Stone, by the Care she has taken to line the Ureters and Bladder with the like Substance, which is separated from its Glands; which serves not only to prevent the Bladder's being injured by the Acrimony of the Urine, but also to hinder the Adhesion of the tartarine Particles of the Urine, which are then found to adhere to the Bladder, when this mucilaginous lining is by the *Calculus* rubbed off: And a *Calculus* Sand is sometimes found adhering to the whole Substance of the Bladder, which grows thereby thick, hard and schirrous. And daily Experience teaches us that the Tartar of Urine constantly adheres to Urinals and other Vessels it is contained in; and would doubtless do the same to the Substance

stance of the Bladder, were it not defended with a slimy Coat. And accordingly it is observed by modern Physicians, that the more mucilaginous the Urine itself is, the less apt it is to breed the Stone: No wonder then, that balsamic, lubricant and mucilaginous Liquor, such as Milk, soft unctuous Ale, Water-Gruel, Barley-Water, and Liquors made of or mixed with Honey, &c. are found to be good Preventives of the Growth of the Gravel or Stone.

8. And as roasted Bread, roasted, broiled and fried Meats of all kinds, are observed to be apter to breed the Stone than boiled, this is doubtless owing to the same Cause, *viz.* that they are less mucilaginous than boiled Meats; for the Surface of these Meats being imbrowned, that is in some Degree burnt, the mucilaginous Texture of these burnt Parts is thereby spoiled, and the Cohesion of the tartarine Air Particles (many of which according to the different Degrees of burning are thrown off into an elastic State) is to such a Degree broken as to leave them more at liberty to form in this their detached unelastic State.

9. Hence

9. Hence we see how greatly the Antients were mistaken who generally attributed the Growth of the *Calculus* to mucose, pituitous Matter, which is sometimes in great Plenty separated from the scrophulous Glands of the Bladder, but is not found to harden into a *Calculus*.

10. It was the general Opinion of the Learned in *Beverovicus's* Time (who published what they wrote to him on that Subject about a hundred Years since) that the Matter of the *Calculus* was a *pituitous* viscid Matter, caused by the ill disposed Kidney, and hardened by the Heat of the Bed. But *Helmont* in his Treatise *de Lithiasi*, justly explodes this Notion, and denies the *Calculus* which he calls *Duelech* to be owing to any viscid Matter for the following Reasons, *viz.*

11. He says the viscid Matter, which is sometimes found in those who have a Stone in the Bladder, arises from the Mucilage which the Stone rubs off the Bladder; for when the *Calculus* is cut out of the Bladder, there is no more viscid Matter in the Urine: That if it arose from this *Mucus*, the Bladder would soon be filled with a *Calculus*: That this *Mucus* dried, makes only a little Chalk Stone,
like

like that of the *Mucus* of the Nose when dried : And that it cannot incrust in the Bladder to form a *Calculus* ; whence he concludes that it does not arise from this *Mucus*, but from a part of the Urine which adheres to Urinals.

12. Tho' he filtrated Urine thro' Linen, yet it still deposited its Tartar, whence he concludes that the Tartar was not formed when the Urine was first voided ; or if it had passed the Linen in the Form of Sand, then it would have precipitated, like Sand or Sediment, to the bottom of the Urinal, and not have adhered to the Sides at equal Distances ; because it would have wanted, he says, a Glue whereby to have adhered to the Sides.

13. And since, as he further observes, this Sand is no where glutinous, but where it adheres to the Vessel, it is, he says, hence evident, that this Sand adheres at the instant it is formed : And this Sand is formed and adheres to the Vessel long after the Urine is made, when it begins to putrify, the very instant before it fixes. To which more and more is continually added. And in the same manner he concludes the *Calculus* to grow. He observes this Tartar to adhere sooner and in greater
Plenty

Plenty to a Vessel already incrusted with it, than to a clean Vessel, *viz.* because the fix'd Tartarine Particles attract more strongly than those of the Sides of the Vessel: An uncomfortable Observation for those to reflect on, who have a *Calculus* once begun in them.

14. He found that even distilled Urine deposited a Tartar; the Reason he gives why Urine long detained in the Bladder does not deposite its Tartar to the Sides of the Bladder, as in a Pot, is because in the Bladder it is not so soon disposed to putrify, and that Tartar will not adhere so well to the Bladder as to a Pot.

15. *Helmont* finding the Ignorance and Incogitancy of others on this Subject, says, he gave away as many Books, as were worth two hundred Pieces of Gold, and wished he had burnt them; for Books gave him not the least Light. A mortifying but strong and lively Instance of the little Progress we must ever hope to make in our Researches into the Nature of Things, without first striking out what Light we can, by variety of proper Experiments.

16. The Urine that is made at the greatest Distance from drinking, is observed to be
 6 most

most rancid, as is in the like Case the Milk of Women; because it has not only been longer a digesting in the Blood, but also because the more watry part goes off first in greatest Proportion: And its being more easily retained in the Bladder, than the higher coloured Urine, shews that it is less rancid and stimulating, than a higher coloured Urine.

17. Whence it seems probable, that the Stone does not increase with an equable Progression, in those that are subject to it, but faster or slower, in proportion to the more diuretic and diluted, or more rancid State of the Urine. Whence we may not unreasonably conclude, that the Stone increases faster in the Summer, when a greater Proportion of Moisture is perspired off, and the Urine rendered thereby more rancid, than in the Winter, when the Perspiration being diminished, a greater Quantity is discharged by Urine. Add to this that Heat is observed to contribute to the Induration of many Bodies. *Aretæus* on the contrary imagined the *Calculus* to be increased most in Winter and Autumn, viz. from a suppressed Perspiration.

18. The *Lamina* or Cloves which are observed in many of the *Calculi*, are a further

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Confirmation of this ; for when the Urine does not deposite its Tartar plentifully on the *Calculus*, then its Surface acquires, by rowling up and down in the Bladder, a kind of hard polish ; but when the Urine returns again, to its more rancid tartarine State, then the *Calculus* is covered with a new Coat ; the last polished Surface, being the Partition between the two Cloves, at which Surface they more easily separate than elsewhere.

EXPERIMENT IX.

1. **I** Put into a *Florence Flask* full of cold Water, a small round redish Gravel Stone, about $\frac{1}{8}$ th of an Inch Diameter, and also a Piece of a very hard *Calculus*, and having suspended the Flask over a Fire, when the Water boiled, Air arose in plenty from the Gravel, whereby it was raised up in the Water, and much agitated to and fro ; so that it looked like the *Nucleus* of a Comet, with its long Train of Air Bubbles arising from it.

2. When it had boiled for an Hour and half, on pouring in a little more hot Water, it ceased to boil for a Minute, and during that time, no Air arose from either of the *Calculi*.

3. An

3. An Hour and half after that, I poured into the Flask some more Water, which was much cooler, than what was poured in before : As soon as the Water boiled, I expected Air would arise again from the Gravel ; but none rose till it boil'd a considerable time : Then I took out the Gravel and found it wasted two thirds : But the very hard *Calculus* was not sensibly wasted, notwithstanding some Air arose from that also in boiling.

4. I repeated the same Experiment, with two other large hard Gravel Stones, and a Piece of *Rhenish Tartar*, which was dissolv'd in a Quarter of an Hour, and found that when the Flask was filled up with very hot Water, then Air arose from the Gravel, soon after the Water began to boil again : But when what was wasted by boiling, was filled up with cold Water, then no Air arose from the Gravel, till it had boiled a long time.

5. Hence we see how the Alternacies of hot and cold, warm and cool, as they fix the Parts of the Gravel and thereby hinder their arising from it in the Form of Air, so they conduce much to the hardening of some Bodies. Thus the Parts of Animals and Vegetables do gradually coalesce, and the *Calculus* hardens

more and more in the Bladder. And thus by Changes from very hot to sudden cold, fatal Coagulations of the Blood are frequently made.

6. If there be any thing in the common Notion, that laying on one's back, contributes to the Increase of Gravel in the *Kidnies*, by heating of them, then this Experiment will shew one Cause which may contribute to it. Tho' I suspect that a principal Cause of the first beginning of the Growth of Gravel in the *Kidnies*, is owing to the horizontal Posture we are in when we lay in Bed : In which Posture one of the *Kidnies* being lower than the *Bladder* when we lay on one Side, and both the *Kidnies* when we lay on our Back ; the *Pelvis* or Cavity of the *Kidnies* becomes thereby the Sink for the tartarine Parts of the Urine to settle in. And the Urine being in that Posture propelled by the *Kidnies* with a Force not only equal to the perpendicular Height of the *Bladder* above them ; but also with a Force sufficient to dilate the *Bladder* and circumambient Parts of the *Abdomen* ; the Urine must needs press (especially when the *Bladder* is in a good Degree full) with a considerable Force against the Orifices of the excretory Guts of the *Kidnies* ; whereby the

Progress

Progress of the Urine being in some degree retarded, it has more time to deposite its *Tartar* in those small *Ducts* in the Papillæ, where it is thought the first minute Beginnings of Gravel are usually formed; it being in Dissections found there.

7. *Qu.* Might not these Inconveniences be in some measure obviated, by laying as Soldiers do in their *Barracks*, not in a horizontal but in a reclined Posture, with the Head and upper parts of the Body considerably higher than the Feet and lower Parts?

8. In this Posture the Urine flowing more easily down the *Ureters*, would not only more readily carry its tartarine Recrement along with it; but its Pressure against the Orifices of the excretory *Ducts* being thereby taken off, it would more readily and in greater Plenty be separated from the Blood: For which Reason it seems not improbable that an erect Posture of the Body may contribute something, among other Causes, that are with good Reason usually assigned, for the greater Flow of Urine in the Day time, than in the Night.

9. The Care Nature has taken to prevent the Urine's regurgitating from the Bladder, by means of Valves placed at the lower Orifices

of the Ureters, where they discharge themselves into the Bladder, shews the Importance of the Urine's not pressing on the Orifices of the excretory Ducts of the *Pelvis*. *Ruyfch* observed the Kidnies of a Sheep extended to the Size of a Pint, occasioned by a Stoppage of Urine in the Bladder; and doubtless, lesser Degrees of Pressure on those *Ducts*, will proportionably incommode their Secretions.

10. And for the same Reasons, it must needs be advicable, to take care to lay alternately sometimes on one Side, and sometimes on the other: For since while we lay, suppose on the left Side, the excretory *Ducts* and *Sinus's* of the Kidney, will be in a low depending Posture, for the Recrement of the Urine to fix in them; while at the same time the excretory *Ducts* and *Sinus's* of the right Kidney will be in such a superior Posture, as that the separated Urine freely flows from them, and *vice versa* when the right Kidney is lowermost. Therefore it seems to be of no small Importance, to change Sides alternately, that whatever Sediment may have been settled in the inferior Kidney, may by inverting it, be washed out, before it have time to fix into small Sand
and

and Gravel, whence it too easily increases into a larger Size.

11. And for the same Reason it seems advisable, that when we feel the first Beginnings of Uneasiness in one Kidney, we should take care to have that be, for the most part, the uppermost Kidney, to try if haply by that Means, the first beginning Sand may be washed away.

12. It is to be suspected that the first Beginnings of *Calculus* in Infants, may be frequently owing to their laying much upon their Backs, as well as to their long retaining of Urine in the Bladder while they sleep much.

13. I am sensible that these may seem to some to be very trifling Precautions; yet for the abovementioned Reasons I cannot think them such; tho' I am far from imagining that they will be an effectual Preventive for all; yet as they may possibly be beneficial to some, it will sure be worth the while to make so easy a Tryal, in a case of so great Importance to our Welfare.

14. *Helmont* says he was once afraid of laying on one Side constantly, lest the Urine should not pass freely from the lower Kidney to the superior Bladder, besides what Impedi-

ment it might suffer from the Pressure of the Bowels. But he was eased, he says, of this Fear, when he found two Persons who lay constantly, the one on the right and the other on the left Side, and yet were neither of them troubled with *Calculi*, in those lower Kidnies, but one of them in the upper. But notwithstanding this and some other like Instances that I have known, yet it seems from the abovementioned Reasons, most likely that the lower Kidney should be most liable to breed Sand and Gravel.

15. Many observing one Kidney to be free from *Calculi* and the other not, concluded it was owing to the different Soundness of the Kidnies; some alledging that the excretory *Ducts* in the morbid Kidney were too narrow, others that it was too much relaxed; which they are often found to be: Which Relaxation may probably be owing to stopping the Secretion of Urine, and to a long Series of Pain from the *Calculus*, not that the Relaxation was the antecedent Cause, but the Effect of the *Calculus*.

EXPERIMENT X.

I. **W**HEN we consider the great Quantities of Air that are in our Food, whether animal or vegetable (without which it would be no Food) and at the same time reflect on the Disposition that most of the Liquors which we drink have to deposite tartarine Concretions, we cannot wonder to see the Urine of some Persons so much disposed to breed the *Calculus* : Which Quality of the Urine seems to arise in a greater Measure from our Drink than from our Food. Our Food, especially that which is boiled, being more mucilaginous than the greatest part of the Liquors which we drink, and consequently less apt to deposite its *Tartar* ; of which Conjecture we have some further Confirmation from hence, *viz.* that by comparing the several Airs generated by Distillation, Fermentation, or Effervescence, from animal and vegetable Substances, with the Airs generated from their *Tartars*, which are Concretions mostly from their fluid Parts, we find that the *Tartars* yield considerably more Air, than the solid Substances of Animals or Vegetables :
Which

Which shews the Aptness of fluid Mediums, for tartarine Concretions to be formed in. And this holds true not only in Urine, and fermented Liquors, as Wine, &c. but the same may be observed in most Waters. Thus I find upon tryal that the Incrustations of petrifying Springs are of a tartarine Nature; as are also the Incrustations, which adhere to the Bottom and Sides of Vessels in which Water has been frequently boiled.

2. For three hundred and eighteen Grains, or about half a cubic Inch of petrified Incrustation from the *Cold Bath* in *Madingly Wood* near *Cambridge* being distilled in the Iron Retort, *Fig. 38. p. 266. Vol. 1st.* there arose from it three hundred and twenty six cubic Inches of Air, of which fifty four Cubes lost its Elasticity in six Days.

3. And from a hundred and six Grains of the same Substance mingled with Spirit of Salt, there arose in Fermentation seventy two Cubes of Air, all which in seven Days lost its Elasticity.

4. And it was the same with the Incrustation of a Tea Kettle, in which Well Water had been boiled, which Well was dug below a *Stratum* of Gravel into a blue Clay: From

$\frac{1}{4}$ ths of a cubic Inch of this Incrustation I obtained by Distillation three hundred and twenty four cubic Inches of Air, of which one hundred and eighty lost their Elasticity in four Days.

5. And from a like Quantity of the much harder Incrustation of the *New River* Water there arose in Distillation two hundred and thirty four Cubes, of which in four Days a hundred and eight lost their Elasticity.

6. The Air arises much slower from these Substances in Distillation, than from the *Tartar* of Wine or from the *Calculus Humanus*, for which reason a longer Continuance of an intense Degree of Heat is requisite.

7. From three hundred and twenty eight Grains, or about half a cubic Inch of the powdered Incrustation of boiled Well Water, let fall on two Cubes of Spirit of Salt, there arose eighty one cubic Inches of Air, all which lost its Elasticity in seven Days.

8. Three hundred and twenty eight Grains of the last mentioned Incrustation, with two Cubes of Oil of Sulphur, generated two hundred and sixteen Cubes of Air, all which did also lose its Elasticity in seven Days.

9. The same with the like Quantity of Oil of Vitriol generated a hundred and ninety eight Cubes, of which a hundred and twenty four were resorbed in seven Days.

10. A hundred and forty six Grains of Incrustation of a Tea Kettle from Water that came from Chalk at *Basingstoke* in *Hampshire*, generated with Spirit of Salt a hundred and twenty six cubic Inches of Air, of which seventy two lost their Elasticity in seven Days. This Incrustation was deposited in such Plenty from the Water, that in the Space of two Years the Crust was half an Inch thick.

11. Hence we see that these Incrustations are of a tartarine Nature: They adhere also like *Tartar*, not only as Sediment to the bottom but also to the Sides of Vessels: Whence we may not unreasonably conclude, that many Waters which produce these, have Principles in them, which may promote the Growth of the Stone in the *Kidnies* and *Bladder*. For which Quality some Waters are more remarkable than others, as those of *Paris*, which so fur up the Pipes, with these tartarine Incrustations that the Water cannot pass. And it is well known that the Inhabitants of *Paris* are more subject to the Stone
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in the Bladder than those of most other Cities. An Argument that Liquors contribute more to the breeding of the *Calculus* than our Food. Of which we have a further Proof in the Instance of small Wines, which abound with *Tartar*, and which are too well known to subject those who drink them to the Stone and Gout.

EXPERIMENT XI.

1. **T**HAT Waters are impregnated with these tartarine Particles, more or less according to the different Nature of the several *Strata* or Beds of Minerals, Stone, &c. which they percolate thro', is evident from the following Experiments, *viz.*

2. A cubic Inch of *blue Clay* being distilled, a hundred and eight cubic Inches of Air were raised from it, of which thirty six Cubes lost their Elasticity. This Clay does not ferment with Spirit of Salt.

3. Three hundred and eighteen Grains of white *Italian Marble* being distilled, little Air arose, till it had a very great Heat, when two hundred and thirty four cubic Inches of Air were generated, of which fifty six Cubes lost their Elasticity in five Days,

4. And

4. And from the same Quantity of a transparent *Trapezium* like Spar, which came from the Mountains in *Switzerland*, two hundred and eighty eight Cubes of Air were raised, of which ninety Cubes lost their Elasticity in five Days. It was observable that the Air Bubbles which retained their Form for some time, did when their viscid covering was burst, emit Smoak, in the same manner as distilled Hart's-horn did in *Exper. 77. Vol. 1st. p. 189.*

5. From a like Quantity of *Selenites* there arose in Distillation, but thirty nine cubic Inches of Air, of which nine lost their Elasticity in five Days.

6. From one hundred and forty six Grains, or near $\frac{1}{3}$ d of a cubic Inch of *Chalk*, there arose in Effervescence, with two Cubes of Spirit of Salt, eighty one Cubes of Air, of which thirty six lost its Elasticity in nine Days.

7. From a like Quantity of a *Corraline Stone* and Spirit of Salt arose two hundred and eighty eight cubic Inches of Air, of which a hundred and sixty two lost their Elasticity in seven Days.

8. From *Rurbeck Stone* with Spirit of Salt a hundred and eighteen Cubes of Air, the greatest Part of which lost its Elasticity in seven Days.

9. From

9. From *Fire Stone* and Spirit of Salt were generated a hundred and eight cubic Inches, of which thirty six lost their Elasticity in seven Days.

10. In like manner I found great Quantities of Air arise by Effervescence from many other fossile mineral Bodies : As from *Portland Stone*, black Marble, blueish Marble, redish Marble, *Bristol Diamond*, and the Lime-stone Marble in which it grows, also five different kinds of Spars, and a hard *Fluor*, two Stones that look'd like petrified Wood, and two others that resembled petrefied Bones. But no Air arose from a hard redish paving Stone, which is brought in Ballast from *Norway*; nor from the *Darby*, *Collen* or *French Mill Stones*, nor from Iron Oar Stone.

11. Thus we find tartarine Principles in most kinds of fossile Bodies ; no wonder then that the Generality of Waters, which flow thro' them, are impregnated in some Measure with the like alkaline Qualities, so that those mineral Waters which have wrongly been called *Acidulæ*, being found on further Examination to be alkaline, should rather be called *Alkallulæ*. Tho' it is probable that most of the harder Bodies, such as *Bristol Marble*,

Spar and the like, communicate but little of these Qualities, in comparison of what some softer kinds of Stone, Chalk, blue Clay or the like do.

12. Yet there are some Waters which deposite no tartarine Incrustation in the Vessels they are boil'd in; such is the Water which is convey'd in a Pipe, for the publick Use of the Inhabitants of *Hodsdon* in *Hertfordshire*, which rises from a Gravel, and boils up thro' a fine white Sand; which has no Incrustation in a Boiler that has been used for fifteen Years: And such is the Water with which Mr. Serjeant *Baynes's* finely situated House at *Havering* Hill in *Essex* is supplied with; the top of that Hill whence it flows, and on which the ancient royal Bower stood, being Gravel; whence as also from Sand the purest Spring Water has been observed to flow, if it have not first passed thro' *Strata* that it can dissolve. Such also is the Water which his *Majesty's Palace* at *Hampton Court* is supplied with, which leaves no Incrustation in the Coffee-house Boiler, that has been in constant Use for fourteen Years: It has the same Quality at the Fountain Head at Mr. *Harvey's* at *Comb*: And it is the same with the
Waters,

Waters, which arise at *North Homes* and in *Old Park*, which supply the *Dean* and *Prebendaries* and other Inhabitants of *Canterbury*. These Waters come from gravelly Hills, and are conveyed thence in leaden Pipes, one from *Comb Hill* in *Surrey*, and the other from a like Hill about a Quarter of a Mile's Distance from *Canterbury*. So that Water seems not to contract any tartarine Quality from Gravel. And it is accordingly remarkable, that I cannot find upon tryal, any tartarine Quality in *Gravel* Stones or Flint, either by Fire or Fermentation.

13. *Hippocrates* condemns Water that is carried far in Pipes of Lead, yet three of these abovementioned Waters are carried a considerable way in leaden Pipes, that of *Hampton-Court*, about two Miles, and yet none of them have any incrusting Quality.

14. The *Comb* Water is observed to be softer, and to wash Linen clean with a less Quantity of Soap than either *Thames* Water, or the Water of the River, which runs a-cross *Hounslow Heath* to *Hampton Court*. Whence it seems not improbable, that the Hardness of many Waters, and their curdling and coagulating of Soap may be in a good Measure

owing to the tartarine Quality with which they are impregnated.

15. The *Comb* Water is not filtrated thro' a great Depth of Earth before it comes to the Gravel, which is also the Case of the *Canterbury* Waters; and of those at *Havering Bower*.

16. As *Comb-Hill* is Gravel in a manner to the Surface, and the Springs (many of which I have seen opened) arise at the Brow of the Hill out of that Gravel, the Water must partake greatly of the Nature of Rain Water; since the Dew and Rain which falls on that Hill, receives probably no other Alteration, in percolating thro' the Gravel only, than in coming out more pure and free from Sulphur than Rain Water is.

17. On comparing the Quantity of Sediment remaining after the Evaporation of equal Quantities, *viz.* thirty four cubic Inches of Rain Water and *Comb* Water, I found it exactly the same, *viz.* two Grains, which is in proportion to the Weight of Water as 1 to 4445: And the Sediment of *Havering Bower* Water was nearly in the same Proportion. The Method I made use of to do this with Accuracy was by cutting off the upper part of

Florence

Florence Flasks to a large Orifice, which being first weighed were filled with equal Quantities of Water which was also weighed, these being set on a Sand Furnace were there evaporated with a gradual equal Heat. The Sediment of the Rain Water was of a darker brown Colour than that of *Comb* Water; this last in a few Days melted so as to stand in small Drops, whence 'tis likely there may be a very small Degree of Salt in it, and that probably of a nitrous kind. From a like Quantity of *Scarborough Spaw* Water I procured forty eight Grains of Sediment, *i. e.* in the Proportion of one to a hundred and eighty five; it was near as white as double refined Loaf Sugar, in standing some Days it melted and had a very nauseous bitter Taste, as that of *Ebsham* and the like purging Waters have. The Sediment from a like Quantity of *Bristol* hot Well Water was four Grains, *i. e.* in the Proportion of 1 to 2222; it shot into *Stria* like the *Scarborough Spaw* Water, and was white like it; in standing some Days it melted, but had no disagreeable Taste. On the Evaporation of half a Pound of *Havering* Well purging Water there remained $24 + \frac{1}{2}$ Grains of Sediment, *i. e.* as one to a hundred

and forty three. From half a Pound of *Ac-ton* Water there remained twenty two Grains of very white Salt, *i. e.* in the Proportion of 1 to 159.2. And from a like Quantity of *Ebsham* Water seventeen Grains, *i. e.* in the Proportion of 1 to 206.1.

18. It is observed of most Spring and Well Waters, that the fuller the Springs are, by great Falls of Rain, the softer they are: But when the Springs are at any time much fallen, for want of a Supply of Rain, then their Waters return to their former Hardness, which Quality we see they are impregnated with from the blue Clays, and other *Stratums* which they pass thro'. Whence we see the Reason of the common Observation, *viz.* that the Water of Springs when they are very low, is not so good to brew with, as the Water of the same Springs when they are high and full.

19. Dr. *Mead* in his *Treatise of Poisons* takes notice of a common Abuse committed about *London*, *viz.* “ in the choosing of stagnating
 “ impure *Well Water* for the brewing of *Beer*,
 “ and making other Drinks. Such a Fluid
 “ indeed has oftentimes a greater Force and
 “ Aptness, to extract the Tincture out of
 “ *Malt*, than is to be had in the more inno-
 “ cent

cent and soft Liquor of Rivers; but for this
 very Reason it ought not, unless upon meer
 Necessity, to be made use of; this Quality be-
 ing owing to the *Mineral* Particles and *Alu-*
minous Salts, with which it is impregnated.
 20. " A late *Author* (says the Dr.) viz. *Dr.*
J. H. Scelera Aquarum, or a Supplement to
Mr. Grant on the Bills of Mortality, by search-
 ing into the first Accounts of the Distem-
 per we call the *Scurvey*, described by *Pliny*
Lib. 25. c. 3. and *Strabo Geogr. Lib. 6.*
 under the promiscuous Names of *Stomacace*
 and *Scelotyrbe*; and examining the authentic
 Histories of it, in later Years, made by the
 most observing Physicians in those Countries
 where it was unhappily revived, as *Olaus*
Magnus, Balduinus Ronsens, J. Wierus,
Solomon Albertus, &c. finds that the Ori-
 gine of it, was in all Times and Places
 charged upon the Use of unwholsome stag-
 nating *Waters*. Then by comparing toge-
 ther the *Clayie Strata* of the Earth about
 the Cities of *London, Paris* and *Amster-*
dam, he shews that where the Water is
 worst, there this Malady is most rife. So
 that he has put it out of all doubt, that
 most of the perplexed and complicated

“ Symptoms which are ranged under this one
 “ general Name, if they do not entirely owe
 “ their Birth to the Malignity of this Element,
 “ do however acknowledge it to be their
 “ main and principal Cause.

21. “ And indeed *Hippocrates* himself, as
 “ he has very plainly decypher’d this Disease
 “ (*Prorrhēt.* l. 2. c. 16.) by the Title of
 “ *σπλῆνες μέγαλοι*, or *great Milts*; so he does
 “ very particularly in another *Treatise* (*de Aere,*
 “ *Aquis & Locis, sub finem*) take notice, that
 “ drinking of *stagnating Well Waters*, must
 “ necessarily induce an ill Disposition both of
 “ the *Milt and Belly*.

22. “ If we enquire into the Reason of such
 “ ill Effects, we must consider that *Clay* is a
 “ *Mineral Glebe*, and that the gross Particles
 “ and metallick Salts with which Waters pas-
 “ sing thro’ such a bottom do abound, are, as
 “ *Dr. Lister* observes, not to be mastered, that
 “ they are indigestible in the human Body.
 “ *De Fontib. Med. Angl. P. 2. pag. 75.* Not
 “ only therefore will these cause, as he very
 “ well argues, *calculous Concretions* in the
 “ *Kidnies, Bladder and Joints*; and as *Hippo-*
 “ *crates* experienced, hard Swellings in the
 “ *Spleen*; but they must necessarily oftentimes
 “ by

“ by their corrosive Quality twitch and irri-
 “ rate the sensible Membranes of the Stomach
 “ and Bowels, and thus hinder and interrupt
 “ the Digestion of our Food. Nay besides
 “ all this, when they come into the Blood, it
 “ is no wonder if the small Canals of insensi-
 “ ble Perspiration are frequently stop’d and
 “ obstructed by ’em; for it is upon this Score
 “ that *Sanctorius* teaches us, *heavy Water*
 “ *converts the Matter of Transpiration into an*
 “ *Ichor, which being retained, induces a Ca-*
 “ *chexy.* *Medicin. Static.* Sect. 2. Aphor. 6.

23. “ What Mischiefs will insue hereupon
 “ every ones sees; not only Pains in the Limbs,
 “ livid Spots in the Surface of the Body, Ul-
 “ cers, &c. from the Acrimony of the undi-
 “ charged Moisture; but many besides of those
 “ perplexing Symptoms which go by the Name
 “ of *Hysterical* and *Hypochondriacal* may take
 “ their Rise from the same Source.

24. “ If these Inconveniencies are often-
 “ times not felt, at least not till towards the
 “ declining Age, in strong and active Habits
 “ of Body; yet I am, from very good Expe-
 “ rience, assured, that they deserve Consid-
 “ eration in weaker Constitutions, and a se-
 “ dentary Life, especially of the more tender
 “ Sex.

25. “ I have the Honour to be nearly related
 “ to a worthy Person, who led formerly an
 “ afflicted Life from the frequent Returns of
 “ violent *Colic Pains*, till she was with hap-
 “ py Success advised by the noble *Van Hel-*
 “ *mont* not to drink (as she then did) Beer
 “ brewed with *Well Water*; and her Health
 “ is even now so far owing to this Manage-
 “ ment, that an Error in it is unavoidably
 “ followed with the wonted Complaints.

26. “ For these Reasons *Pliny* tells us, *lib.*
 “ 31. c. 3. that *those Waters are condemned*
 “ *in the first Place, which when boiled do in-*
 “ *crustate the Sides of the Vessels*; and that
 “ our *Well Waters* do this, no body who
 “ looks into the *Tea-Kettles* of our Gentle-
 “ women can be ignorant.

27. *Helmont* in his *Lithiasis* mentions a petrifying Spring near *Bruxells*, which subjects the Monks to the Gripes, unless they daily use wild Carrot-seed boiled in Beer. There are indeed on the contrary several Instances of Persons using petrifying and incrusting Waters without finding any the like Inconveniences, or being ever subject to the Stone, but we cannot thence reasonably infer that many pernicious Effects are not frequently produced by them.

28. To

28. To conclude, the foregoing Experiments, as they shew us the Nature of the Stone in the *Kidnies* and *Bladder*, and the Causes from whence they arise; so they may probably be of use, if not in leading us to the happy Discovery of a safe Dissolvent, yet at least they may serve to caution us to avoid such things as are most likely to produce them, and direct us in the Use of such Meats and Drinks, as will be most serviceable in preventing its Growth.

29. For tho' notwithstanding the Urines of all Persons have tartarine Matter in them, as is plain from the Adhesions to Urinals, &c. yet far the greatest part of Mankind are never troubled with the Stone; and tho' most Persons have Sand, yet not many have the Stone in the Kidnies, and fewer in the Bladder: It seems therefore not unreasonable to think that if by proper Meat and Drink and other Precautions, the tartarine Quality of their Urine who are subject to the Stone could be somewhat abated; that then, they like others might be free from the first beginnings of it. But when once any Gravel is formed, it then too readily increases. For which reason it is a Matter of great Importance to use all proper Means,

Means, to void it as soon as possible after it is fallen into the Bladder, before it be grown too big to pass. I suspect that a principal Reason why many Stones do not pass off, is this, *viz.* the Stone by stimulating the Neck of the Bladder, causes a frequent Endeavour to make Water, when there is but little in the Bladder, whereby for want of a sufficient Quantity of Urine to impell it forward it is retained: Whereas did the Patient contain his Water till the Bladder was well filled, there would not only be a greater impelling Force to push forward the Stone, but the Sphincter of the Bladder would also be thereby the more dilated, and so make more room for its Passage, especially if the Urine were made mucilaginous by proper Drinks. And in case it should not by this means pass off, but cause a total Suppression of Urine; it is well known that a Surgeon can easily remove it, by means of a Catheter; which Removal may possibly cause it to lay in a fitter Posture to pass off another time.

30. While I was intent upon these Experiments on the *Calculus*, it occurred to my Thoughts, that large Gravel Stones, which often stick for several Days in the *Urethra*, to
the

the great Torment of the Patients; and which they cannot sometimes be delivered from without cutting them out, might be drawn out by the following Instrument, *viz.*

31. I cut off the lower end of a strait *Catheter* which made it a proper *Canula* for a *Stillet* or *Forceps* to pass thro'; the lower end of the *Forceps* was divided into two Springs, like *Tweezers*, whose Ends were turned a little inwards; these Springs were made of such a Degree of Tenderness, and Pliancy, as not to bear too hard against the Sides of the *Urethra*, by their Dilatation.

32. When this Instrument is used, the Springs are drawn up within the *Canula*; which being passed into the *Urethra* as far as to the Stone, the *Canula* must then be drawn back, so far as to give room for the *Forceps* to dilate; which dilated *Forceps* being then thrust down a little further, so as to embrace the Stone, then the *Canula* must again be slid down, to make the *Forceps* take fast hold of the Stone, so as to draw it out.

33. I sent this Instrument to Mr. *Ranby*, to have his Opinion of it, who tells me that upon repeated Tryals he found it extracts these Stones with great Ease and Readiness: And that

that it is so well approved of by other Surgeons, that many of them make use of it.

34. This strait Instrument will therefore serve to extract such Stones as are lodged, after they have passed the Turning at the *Os Pubis*; and I am informed that they are aptest to lodge in those Parts of the *Urethra* which are within the Reach of this strait Instrument. But if it should lodge a little beyond the Turn at the *Os Pubis*, it might probably be practicable to extract them thence by bending this Instrument, as the common *Catheters* are bent; if the Stilllet were Silver it would bend the more easily.

35. Mr. *Ranby* is of opinion that this Instrument may be farther useful, in case of a Stricture or Contraction of any part of the *Urethra*, viz. by thrusting the *Forceps* into that Stricture, where by continuing some time, the constant Tendency of the Springs to dilate will widen the Stricture.




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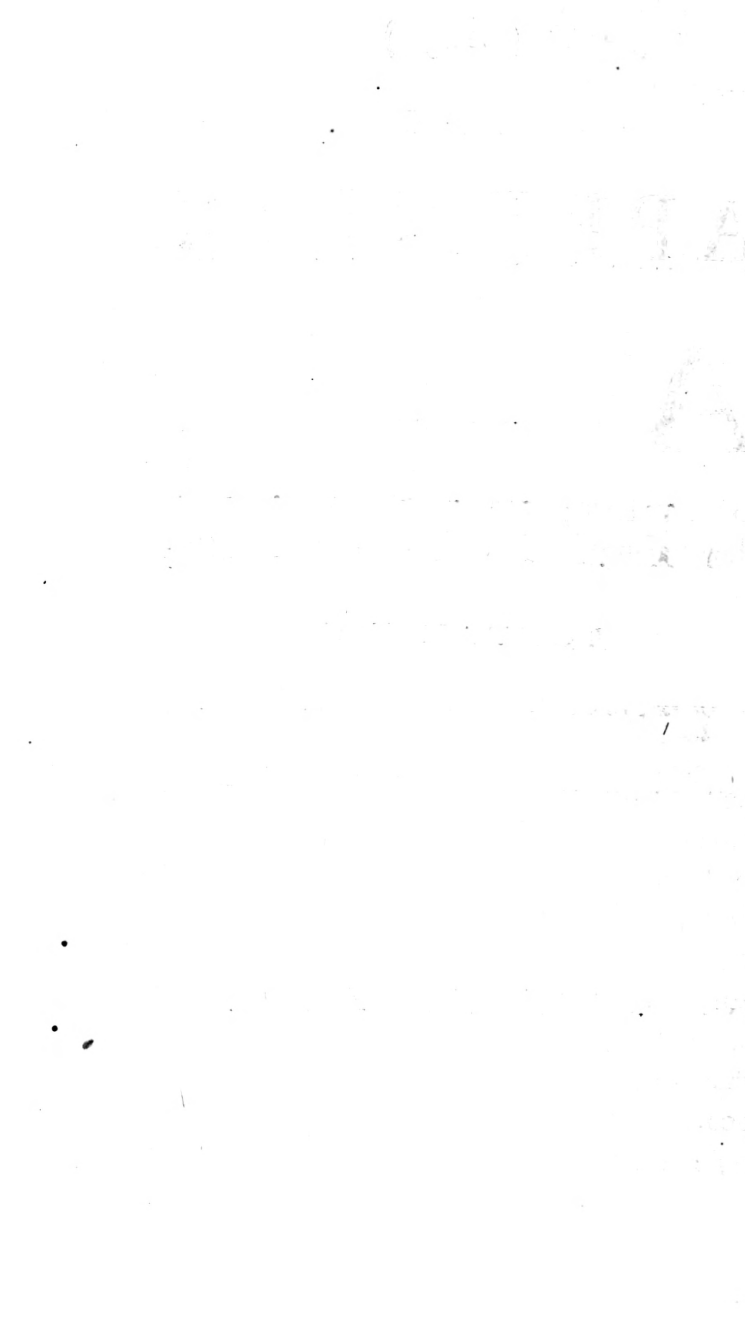
APPENDIX,

CONTAINING

OBSERVATIONS and EXPERIMENTS

Relating to several SUBJECTS in the first
VOLUME.





T H E

A P P E N D I X.

AS these Observations and Experiments were intended to be inserted under their proper Heads in the second Edition of the first Volume, so I have here placed them in the same Order, they having no Connection with each other.

O B S E R V A T I O N I.

I. **H**AVING in *Experiment XIX. Vol. I.* p. 53. found the Quantity of Vapour evaporated from a Surface of Water, in nine Hours in a Winter's-day, to be $\frac{1}{21}$ part of an Inch in Depth ; it gave me occasion to reflect on a vulgar Error, viz. that it is a more unwholesome Situation to dwell on the southern, than on the northern Side of a River or other Water, viz. because it is concluded that the Sun does by its Heat draw the Vapours up towards itself. It is certain that watry Particles do by the Action of Warmth,

rise

rise in the Form of Vapours, perpendicularly upwards, in a still Air. But if that Air be in a progressive State towards any Point whatsoever, the Vapours are then constantly carried along with it, in a more or less oblique Course, according to the different Celerity of the Stream of Air. Heat is so far from attracting watry Vapours towards itself, that it on the contrary repels them every way from itself.

2. What has probably led Men into this Mistake, is the common Observation, that when a wet Cloth is held before a Fire, the Vapours which arise from that Cloth, move constantly towards the Chimney in which the Fire is: Which Tendency is not owing to the Attraction of the Fire, but to the Force of the Stream of fresh Air, which is continually flowing in, to supply the Place of the Air, which being rarified by the Heat of the Fire is incessantly ascending up the Chimney.

3. As it is observed that we have here in *England* more Southerly and South West Winds, than from the opposite Points, so the Vapours must on that Account be blown more to the Northern than to the Southern Sides of Rivers. But the different Degrees of Healthiness of
one

one or the other Side of Rivers, on account of the Vapours which arise from them is so very inconsiderable that it seems to be meer trifling to take any notice of it, nor should I have done it here, had not the contrary been a very prevailing Opinion. The principal Advantage that the North Side of a River has above the South Side, seems to be this, *viz.* that it should be something warmer, on account of the Reflection of the Sun Beams from the Surface of the Water.

O B S E R V A T I O N II.

I. IT was found in the same XIX *Experiment*, Vol. I. on making an Estimate of the Quantity of Dew and Rain which is expended in Vegetation and Evaporation, that much the greatest part of what falls sinks down into the Earth, and is sufficient in Quantity to account for the Origin of Fountains, without having Recourse for Supplies, to the great Abyss of Waters; which is also further confirmed from the following Observations, *viz.* Count Marssilli in his *Histoire de la Mere*, p. 13. observes that Rivers from the neighbouring Mountains of *Languedoc & Provence*,

vence, do discharge themselves by Under-currents into the Sea, and that at great Depths under Water, particularly at *Port Mion*.

2. And I am credibly informed, that Springs which come from the Hills near *Folkstone* in *Kent*, are in like manner seen to boil up, thro' the Sands at the bottom of the Sea. A Proof that the Sea Water does not ascend to the Tops of Hills and Mountains, and there form Springs and Rivers, but that these on the contrary descend into the Sea.

3. If the Sea Water percolated up to the Tops of Hills, then the high Cliffs immediately adjoining to the Sea, should be constantly dripping with this percolated Moisture, whereas they are usually very dry. Particularly in the Isle of *Wight*, there runs a Ridge of Chalk Hills along its Southern Coast with a high steep Cliff immediately bordering on the Sea, which Cliff is constantly dry. But the Springs running as they are determined by the dipping of the *Strata* of those Hills, break out on the Northern Side of them, at a considerable Distance from the Southern Sea, where they form many Rivulets which run into the Sea, on the Northern Coast of the Island. So that we find the Northern Side

of these Hills at a considerable Distance and Height from the Sea, very wet with constantly running Springs, whereas the Southern Cliff which adjoins to the Sea is continually dry, and that just above the immediate Beat of the Waves.

4. It is well known that when large Quantities of Rain fall, that its Waters soak down to great Depths in the Earth, and do there replenish the Springs ; and if Sea Water does the quite contrary, it must be owing to some peculiar Property in that Water, which can cause it to soak upwards to the tops of Mountains which are not only at a very great Distance from the Sea, but also some Miles higher than its Surface at high Water Mark.

O B S E R V A T I O N III.

1. **D**R. *Desaguliers*, in his Abstract of the first Volume of this Book, observes, from the Influence the Sun is found to have in rarifying the Vapours at too Feet Depth under Ground in *Experiment XX. p. 64.* “ That the Heat of the Sun should rarify the “ Moisture of the Earth to this Degree or a “ great deal more, to drive it into the Roots—

“ of Plants, is very probable from some Ob-
 “ servations that Mr. *Beighton*, F. R. S. and
 “ himself made upon the Engine to raise Wa-
 “ ter by Fire, whereby it appears that the
 “ Steam or Vapour of boiling Water is rarer
 “ than Water from which it was produced,
 “ above 13000 times, when its Elasticity is
 “ equal to that of common Air, *Philos. Transf.*
 “ No. 398.

OBSERVATION IV.

1. FROM the Influence the Sun had on
 the sixth Thermometer, at two Feet
 Depth under Ground, so as to raise the Spi-
 rit in it to thirty one Degrees in *Experiment*
XX. p. 64. Vol. I. we may hence see the Rea-
 son why when Cellars whose Walls, or the
 Crowns of their Arches are immediately ex-
 posed to the Sun without any Shelter, they
 will seldom keep Wine or other spirituous Li-
 quors well, *viz.* because they are thereby too
 much heated.

2. I have also observed from those Ther-
 mometers which were fixed at several Depths
 under Ground; that when in *March* the Sun
 has shined bright all Day, it has had a consi-
 derable

derable Influence in warming the Earth to some Depth, notwithstanding a brisk very cold Easterly Wind blew all that Day. And doubtless the Sun has then the like Influence on the inner parts of Trees, whereby it agitates and raises the Sap: As also on the inmost parts of our Bodies when exposed to it, while at the same time the Surface of them is pinched with cold Winds. And if there be any Truth in the common Notion, that sitting still for a considerable time in the Sun shine in such Weather, is apt to cause Agues, this may not improbably be effected by the Influence of two such opposite Degrees of Cold and Heat acting on the Body at the same time, whereby the Blood near the Surface for want of brisker Motion from Action, will be thickened; which is thought to be the State of the Blood when an Ague Fit comes on. And that the Blood moves but slowly near the Surface of our Body, when we sit for a considerable time in the Cold, whether within Doors or without, we may be assured of from common Observation, *viz.* that tho' while we are thus sitting we may not be sensible of any considerable Degree of Cold, yet on our first moving, whereby the Motion of our Blood is

also accelerated, we are then immediately sensible of Chilneſs all over us; which is doubtleſs owing to the very cool Surface-Bloods then running in greater Plenty among the next adjoining inner Veſſels, which being much warmer are then ſenſibly affected, with the Senſation of the much colder Blood from the Surface of the Body.

O B S E R V A T I O N V.

I. **F**ROM the Proof given in *Experiment XL. Vol. I. p. 128.* of a free lateral Communication of the Sap-veſſels in Trees, we may ſee the Reason why, if the Bark be taken, for an Inch Breadth quite round, off from a lateral Branch, it will often kill the Branch below it, tho' on the oppoſite Side, *viz.* becauſe the diſbarked Branch being thereby deprived of a conſiderable part of its Nouriſhment, which it received from between the Bark and the Wood, it muſt neceſſarily draw more vigorously, a greater Quantity of Nouriſhment, thro' its Sap-veſſels in the Wood, than the oppoſite Branch, whereby that Branch being deprived of its due Nouriſhment, will periſh, provided the Size of the diſbarked Branch

Branch be not so small as to deprive the opposite Branch but of little of its Nourishment.

2. I have several times seen instances of this in a large *Catharine Pear* Tree in my Garden, which having on several of its Arms a young thriving Shoot, grown to about two Inches Diameter, this Shoot attracted Nourishment so vigorously, as not only to deprive of due Nourishment and kill the directly strait Branch above it, but also the lateral Branches within eighteen Inches below it, whether on the same or on the opposite Side.

3. And I suspect, that the blasting of here and there a Branch of a Tree, may sometimes be owing to the same Cause, *viz.* the depriving them of Nourishment by the too strong Attraction of the other Boughs. This may sometimes also be owing to a Defect in the particular Root which leads more directly to a dead Branch, as well as sometimes to blasting Qualities in the outward Air, which may be often sufficient to kill Branches that are weakened by these or other internal Causes, and yet at the same time not be able to hurt the more vigorous Branches of the same Tree.

4. And that some Degree of Defect in Nourishment greatly subjects Fruit Trees to
S 4
blasting,

blasting, is evident from common Experience; for when Trees are planted in a poor, or otherwise unkindly Soil, they are so liable to blast, that they very rarely bear any Quantity of Fruit.

O B S E R V A T I O N VI.

I. **W**Hercas in *Experiment XLVI. Vol. I. p. 141.* I have put it only as a Conjecture, that I believed it would be found upon Tryal, that in the early Spring, the Sap moves first in the lower parts of Trees between the Bark and Wood, and not in the upper Part, as it would do, if the Sap ascended thro' the Wood Vessels, and then descended between the Bark and the Wood. And upon Enquiry, I am informed from Workmen that have been long accustomed to bark oaken Timber, that in the beginning of the Season, the Bark often runs well in the Stem, but not in the Branches: And *vice versa* at the latter end of the Season it shall run well in the Branches but not the Stem: And I am pretty well assured from my own Observations, that it is the same in the Vine; whereas the Bark of the Top Branches ought first to be moistened if the Sap descends by the Bark.

O B S E R-

OBSERVATION VII.

1. **T**O other Arguments which are alledged against the Circulation of the Sap in *Experiment XLVI. Vol. I.* this may be added, viz. *Count Marsilli* a curious Enquirer observes in his *Histoire de la Mere*, p. 57. 107. 173, that Sea Plants which are all rootless, except the *Alga*, have no longitudinal capillary Sap Vessels, thro' which rooted Plants draw Nourishment to every part from the Earth. But the whole Substance of these Sea Plants is composed of Vesicles, which Vesicles receive their Nourishment immediately from the surrounding Water, and consequently there being no longitudinal Vessels to convey Sap from one end of the Plants to the other, we may reasonably conclude that there can be no Circulation of the Sap; whence we see that Circulation is not necessary in order to Vegetation.

OBSERVATION VIII.

1. **T**O the Instance of the imbibing Power of grafted Stocks, mentioned in *Experiment*

periment XLVI. *Vol.* I. *p.* 147. we may add that of the imbibing Power of the Shoots of Fig-trees : For if the unripe latter Fruit continue on the Trees all the Winter, they will frequently kill the Shoot they adhere to, as is plain by the Mortifications beginning at the Stem of the Fig, and thence spreading thro' the whole Shoot, whereas other Shoots which had no Figs on them, have at the same time survived, as I have found by repeated Tryals on Fig-trees which stand exposed without the Shelter of a Wall : For which reason it is adviseable to pull off the latter Crop Figs before the Winter comes on, which will be a means to preserve the Shoots in our ordinary Winter ; but in very severe Winters, such as that in the Year 1728, all the bearing Shoots will be killed, unless sheltered in a warm Situation. Now it cannot be supposed that this Mortification was occasioned by a Circulation of the Sap between the bearing Shoot and the dead putrid Fig ; but should rather seem to be effected by the Shoots imbibing noxious Matter from the Fig. I have observed the like Effect from dry rotten Quinces which have hung on the Tree all the Winter. And it is doubtless in the same manner that a Canker spreads its Infection ; the

ill Effects of which are frequently prevented by cutting out the cankered Part.

OBSERVATION IX.

I. **I** Have in many Instances shewn that Air freely enters the Vessels of Trees, and that it is in a great Abundance wrought into their Substance. *Qu.* May not the Use of those spiral Wreaths that are coiled round the Insides of those Vessels which are supposed to be Air Vessels, and which are manifestly to be seen in several Trees, as also in the Leaves of the Vine and of Scabious, may not these be designed by Nature to promote the quicker Ascent of Air, by being in some Measure conformed to its elastick Contortions? For such spiral Wreaths seem to be altogether useless, for promoting the Ascent of any Liquor, as the Sap which ascends most freely thro' innumerable other capillary Vessels which have no such spiral Coils in them. Not that we are to suppose the Air in its elastic State actually to touch, and thereby to be determined in the Course of these Spirals as any Liquor would be. But as the Rays of Light when they are reflected from a solid Body, are found

to

to be reflected without actually touching the reflecting Body in the Point of Reflection; so it is not unreasonable to suppose, that elastic Air may like Light be diverted from one Course, and so be determined to another by the solid Bodies it approaches without touching them, but rebounding like Light from those solid Bodies near the Point of Contact.

2. I observe that these Spirals are coiled in a Course opposite to the Course of the Sun, *viz.* from West to East.

3. I have several times observed, that when the Fumes of burning Brimstone have ascended among the Leaves of a Tree, those Leaves have very soon withered; and that, not by any scorching Heat of those Fumes, for the burning Brimstone was at too great a Distance to have that Effect; whence it seems probable that these Fumes fix and destroy the Elasticity of some active elastic Principles in the Leaves: And *Qu.* may not some Blasts be owing to the like Effect, of Volumes of sulphureous Vapours which may float in the Air?

EXPERIMENT I.

1. **I**N *Experiment* LXVI. *Vol.* I. *p.* 181. I have given an Account of the Method I used to try what Proportion of Air could be obtained from Brandy, common Water, Rain, *Holt*, *Bristol* and *Piermont* Waters. I procured in like manner a good Quantity of Air from *Spaw* and *Tunbridge* Waters.

2. 'Tis observed that when these Waters are deprived of this subtile elastic Matter, which is impregnated with a sulphureous vi-
triolic Spirit ; they then lose their mineral Virtue and Effects, and will neither tinge with Galls or Syrup of Violets ; and are also thereby deprived of their good Efficacy to those who drink them.

3. I found by the like Tryals on *Ebsham* and *Acton* Waters, that they had little more of this elastic Matter than common Waters ; doubtless it is the same with *Scarborough*, *Stretham*, and the like purging Waters. The Airs of some of these mineral Waters, either lost their Elasticity or were resorbed again into the Water in standing two or three Days ; but a great part of that of *Ebsham* and *Acton*
continued

continued for some Weeks before the Bubble which they formed on the top of the Water wholly disappeared.

4. But from near two Quarts of *Bath Water*, I could scarce obtain a Quantity of Air equal to the Size of half a Pea, which may be owing both to its Warmth which will cause the elastic Spirit to fly off, and also to its subtile Sulphur which may fix that Spirit.

5. I heated some of this Water in an inverted Vessel, which was half full of common Air, to try if the Fumes would absorb any of it, but when the Vessel was cold, I found none of it absorbed. Whence we see, that when *Bath Water* cures Flatulencies in the Stomach, it does not do it by absorbing the flatulent Air generated in the Stomach, but seems rather to effect it, by preventing the arising of much Air from the Contents of the Stomach, by means of the subtile Sulphur contained in it, in the same manner as the Fumes of Brimstone do effectually check the Ferment of spirituous Liquors. And since the strongest sulphureous Fumes, such as those which arise from burning Brimstone, as also from the violent Ferment, which is made by Spirit of Nitre, poured on powdered vitriolic
Stones,

Stones, since these Fumes are not found to absorb the half of any inclosed Quantity of Air, there is therefore little Reason to expect that any the most prevalent Medicine should cure these Flatulencies, by absorbing what Air is already generated. We may rather conclude that it is effected by preventing, by means of the sulphureous Quality of the Medicine, the Air's rising too freely from the digesting Contents of the Stomach.

6. And perhaps its Effect on the Blood may be somewhat of the same Nature, *viz.* to strengthen its Texture by its fine sulphureous and chalybeate Particles, which are also of the sulphureous kind; whence probably less flatulent Secretions will be made from it into the Stomach and Bowels.

7. And as Rain Water is known to be impregnated with a fine Sulphur, especially in warm Weather, it should therefore be better than other common Water for the Flatulent to use: And if Rain Water stands some time to settle, and be then drawn into another Vessel, 'tis said that it will continue sweet and fit to drink for a long time.

8. As this Experiment on *Bath* Water, and the obvious Deductions from it, carry me no farther,

farther, so I do not pretend to trace what other Effects these kinds of Waters may have; by rectifying in other Respects the Blood and Spirits, or by strengthening the too relaxed Fibres of the Stomach and other parts of the Body: For *cæteris paribus* more Air will arise from the digesting Ailments in a relaxed State of the Stomach, than when in a healthy firm State; it more vigorously embraces its Contents, in the same manner as fermenting Liquors emit more Air in open Vessels, than when close confined.

EXPERIMENT II

1. **A** Florence Flask being filled with Ale, and its Nose inverted into a small Vessel full of the same Liquor; when about two cubic Inches of Air were risen out of the Ale to the top of the inverted Flask, I poured that Air up into another inverted Flask full of Water: In ten Hours the greatest part of this Air had either lost its Elasticity or was in part absorbed by the Water, of which little remained the next Day.

2. Hence we see that while Air is rising from Ale and other fermenting Liquors, some

of that Air is returning at the same time from an elastic to a fixed State: And to find out whether it was so or no, it was necessary to separate in this manner, a Proportion of this Air from the Ale, which by continually sending up fresh Supplies of Air, made it difficult otherwise to distinguish whether it were so or no.

3. From this Experiment on Ale we see the Reason why many of those fermenting Mixtures mentioned in *Vol. I. Chap. VI.* were found to be sometimes in a generating and sometimes in an absorbing State, *viz.* because sometimes they generated more than they resorbed, and then they appeared to be in a generating State; and sometimes, especially on a Change from a warmer to cooler State, they then appeared to be in a resorbing State, not that their resorbing Power was greater in the cooler State, but because little fresh Air was then generated, and part of what was before generated, was continually losing its Elasticity.

4. By several other the like Experiments I found that all the Air of fermenting Mal, Drink did not lose its Elasticity, nor was resorbed by the Water, and that, whether I

used large or small Quantities of this Air, with either Rain or common Water, fresh or salted, unboiled or boiled, to clear it of its latent Air.

5. Neither, as was observed in the foregoing Experiment, will all the latent Air which is by Heat raised out of several Waters, be reformed again, or lose its Elasticity till after several Weeks, as was the Case of the Airs from the *Ebbsam* and *Aetou* Waters. But as to the *Pymont*, *Spaw* and *Tunbridge* Waters, whence a greater Plenty of this elastic Matter is raised by Heat, it does not continue so permanently elastic; which may be the Reason why several of these mineral Waters are observed to grow vapid and lose their medicinal Spirit in standing some time, tho' well corked up in Bottles, and even tho' put into a *Florence* Flask which was hermetically sealed, as I was told by the late Dr. *James Keill* who tried the Experiment on a mineral Water near *Northampton*: So that these Waters may be thus deprived of what is called their Spirit, not only by its flying away, as it will soon do in open Vessels, or when heated; but also by its being reduced from an elastic spirituous to a fixed State in the Water.

6. We may hence reasonably conclude that Waters and most other Fluids contain in them both elastic and unelastic Particles; the elastic Particles readily expanding into Bubbles of a visible Size, when the Weight of the incumbent Atmosphere is taken off by the Air Pump. But the Quantity of Air which arises by this means, or by Heat, is but small in comparison of the Bulk of Water. Yet Monsieur *Mariotte* thought he had raised by Heat, from a drop of Water, a Quantity of Air, equal to eight or ten times the Bulk of the Drop of Water. *Essay de la Nature de l'Air*, p. 111. The Air which he raised by Heat arose doubtless from the Oil which surrounded the Drop of Water, for Oil abounds with elastic Air as I found by *Experiment LXII. Vol. 1st. p. 179.* In my *Experiment LXVI. Vol. 1st. p. 181.* I gave the Water a scalding Heat, if the Heat had been greater the Water would have expanded, so as to have either blown up the inverted Vessel, or to have been all expelled forcibly out of it, as would have been the Case with Monsieur *Mariotte's* Drop of Water at the Bottom of his inverted Glass Thimble, not only full of Oil, but also surrounded with it; if he had given it a greater Heat than I

gave the Water in my inverted Vessel.

7. That the Air which is raised and separated from Fluids takes up a much greater Space, and is consequently more expanded than while in the Fluid, I found by the following Experiment, *viz.* I cemented to the Mouth of a half Pint Viol, a Glass Tube of three Inches Length, and half an Inch Diameter in bore : Then filling the Viol full of small Beer, I put it in a Glass Vessel which was ten Inches deep, filling the Vessel with Water ; I then placed over the Mouth of the Viol a small inverted Glass Tunnel, made of the Neck of a *Florence* Flask, its small Orifice being stopped with a Cork, which Cork rested on the Mouth of the Viol. Then by reclining the deep Glass Vessel, all the Air bubbled out from under the Bottom of the Viol, and from under the inverted Tunnel. Then placing the whole under an Air Pump Receiver, I exhausted gradually so much Air, as to cause Bubbles to rise out of the Beer into the Glass Tunnel, sufficient to occupy a Space nearly equal to a cubic Inch. Then letting in the small Quantity of Air which had been exhausted out of the Receiver, I found the Quantity of Air in the inverted Tunnel to be many times greater

er than the small empty Space in the Tube which was fixed to the Viol, which was wanting to be filled up with Beer, and which Vacancy was in a great Measure occasioned by the Waste of Beer, which was made by the Froth of the Bubbles, which flowed over the Mouth of the Viol. Whence it is evident that this Air takes up much more Space than when interspersed in the Liquor. And in *Lowthorp's Abridgment of the Philos. Transf. V. 2. p. 219.* it was found that on exhausting the Air Bubbles out of Water, the Bulk of the Water was scarce sensibly diminished. Yet it cannot hence be inferred that the Air was not in an elastic State while in these Liquors. That there is elastic Air in common Water is certain from hence, *viz.* because when frozen to Ice, the Air Bubbles are then by the uniting of many of them together, become large and visible; yet Cold as 'tis well known does not increase but constantly Decrease the Expansiveness of elastic Air.

8. Some have attributed the Expansion of Ice in freezing to the large Collections of these Air Bubbles, which are not to be seen when the Ice first freezes, but are observed daily to increase in Bulk more and more; yet the Air

in these Bubbles is not forcibly pent up in them, as I found by the following Experiment, *viz.* having placed a Piece of Ice under Water, I pierced Holes into several of the Bubbles, yet the Air did not rush out with any Force, which it would have done, if it had been in a strongly expansive State.

9. But tho' a good part of the Air, which thus arises from Fluids, seems to have existed in an elastic State in those Fluids; yet the Air which arises from solid Bodies either by the Force of Fire, or of Effervescence, does not seem to arise only from the Interstices of those Bodies, but principally from the most fixed Parts of them. For since the Airs which are raised by the same acid Spirit from a great Variety of Substances have very different Degrees of Permanency, as it was found in *Experiment X. Numb. 3, 4, 5, 6, &c.* and in *Experiment XI. Numb. 6, 7, 8, 9, 10. of Experiments on Stones*; hence 'tis probable that these Airs do not arise from latent Interstices of the dissolved Stones, &c. but from the solid fixed Substance of them. And since the whole of some of these new generated Airs, does in a few Days lose its Elasticity, it should seem hence probable that whatever Air arises from

the Spirit in the Effervescence is not permanently elastic, or else that in the Solution of some Stones, it is thrown off into a more permanently elastic State than from others.

10. And that this Air, which is thus generated from solid Bodies by Effervescence, is not meer latent interstice Air, seems further probable from hence, *viz.* that Tartar which abounds so much with Air when dissolved with Spirit of Nitre in *Experiment I. Numb. 4. of Experiments on Stones*, generated no Air, which shews that certain effervescent or fermenting Degrees of Vibration are requisite, in order to throw the Particles of dissolving Bodies into a permanently elastic State.

11. There are other Instances in Nature, where the same Particles are sometimes in an elastic, and at other times in a fixed State: Thus in the Experiments on Electricity, the same Particles of Tinsel, Down, &c. are when approached by a well rubbed glass Tube, sometimes in a repulsive, *i. e.* elastic State, and sometimes in an attractive and cohering State. We may observe the same also in Water, whose Particles in a hot explosive State are vastly elastic, but in a frozen State, strongly cohering and fixed; and why may not Air Particles have

the same Properties? For as all the Parts of this System are in a constant oscillatory Motion, so all Matter seems to be agitated by a repulsive and attractive Force.

12. But it is observable, that permanent Air arises chiefly and in greatest Plenty, from Bodies which are specifically heavier than Water, whose Attraction in a fixed State, and Repulsion in an elastic State, being on that Account greater than that of the lighter watry Particles, they are therefore better adapted to be (with the sulphureous Particles, which they most strongly attract) the principal Band of Union of solid Bodies, than the lighter watry Particles: Tho' doubtless all the Particles of Matter whatever do in actual Contact cohere, yet since it is found by *Experiments XLIX and LV. Vol. I.* that the most solid Parts of Animals and Vegetables, yield a vastly greater Quantity of Air and less Water, than the more lax and fluid Parts, which abound with a watry Fluid; it seems therefore hence reasonable to conclude, that their Solidity is principally owing, not to the watry, but to the Air and sulphureous Particles.

13. And the same holds true, when we consider these Particles in an elastic State, for
the

the specifically heavier Air Particles continue in a much more permanently elastic State, than the lighter warry Particles; which tho' probably, by reason of the much greater Number of them in equal Bulks, do make a vast Explosion when heated to a certain Degree, yet on the Abatement of that Heat, they instantly lose their Elasticity.

14. It will puzzle the *Epicureans* to give a rational Account how from a Chaos, a meer Necessity of Nature, and the casual Concourse of Atoms, so considerable a Quantity of Matter, as we find plentifully interspersed in animal, vegetable and mineral Bodies, should be endued with this double Capacity, of changing *pro re nata* from a strongly attracting fixed State, to a permanently and vastly elastic State, and *vice versa*; this wonderful Property of it, which is so necessary for carrying on the constant regular Processes of Nature, must needs be owing to the Direction of an allwise Being.

O B S E R V A T I O N X.

1. **W**HEN I would distill Tartar, or any the like Substances which abound with great Quantities of Air, I have found it an

an effectual Way to prevent the Bursting of Receivers and Retorts, to lute at the Junction a glass Tube eight or ten Feet long, and half an Inch in Diameter, thro' which the Air which arises in Distillation passes; the Length of the Tube at the same time preventing, in a good Measure, the flying off of the fine volatile Parts. By means of this Precaution a Retort may also with Safety be filled fuller of such windy Substances as will not boil over without any Danger of bursting the Glasses.

OBSERVATION XI.

1. **W**HEN in *Experiment LXXIV. Vol. I. p. 184.* half a cubic Inch of *Sal Tartar* was distilled from an animal *Calx*, there arose from it two hundred and twenty four times its Bulk of Air, and the remaining Scoria did not run *per deliquium*, an evident Proof that all the *Sal Tartar* was distilled over: Which shews that *Sal Tartar* consists of volatile Salt, firmly combined with Air Particles by the Action of Fire. For in the Dissolution of the Parts of a vegetable Substance by Fire, a considerable Quantity of volatile Salt flies off into the open Air; while at the same time,

time, some of it is reduced to a fixed State, by being strongly united in the Operation to Particles which are capable of being thrown into permanently elastic Air. This is very frequently observed in the making of Charcoal; the Dust which covers the great Pile of burning Charcoal, being covered with the white volatile Salt which arises from the Wood; while at the same time another part of the volatile Salt which is found in the Form of *Sal Tartar* in the Ashes of the Charcoal, is reduced to so fixed a State, that it is very difficult to volatilize it, unless it be mixed as in this Experiment with a *Calx*.

E X P E R I M E N T III.

I. **I**N *Experiment* XCVI. *Vol.* I. *p.* 224. I observed, when I let fresh Air into the glass Vessel, *ay* *Fig.* 34. *p.* 168. that the sulphureous Vapours which arose from the Mixture of Spirit of Nitre and vitriolic *Walton* Mineral, did absorb the fresh admitted Air so fast as to make the Water rise very visibly in the inverted Glass *ay*; but I did not then pursue the Experiment any farther, which I have since done.

2. For

2. For if after the Effervescence was over when the Air *az* was grown clear, fresh Air was then let into the inverted Glass *ay*, there would immediately arise a violent Agitation between the two Airs, and they would become, from transparent and clear, a redish turbid Fume; during which Effervescence a Quantity of Air, nearly equal to what fresh Air was let in, would be absorbed; and if after this Effervescence was over, and the turbid Air was again become clear, fresh Air was let in, it would again grow turbid and absorb Air as before, and that after several repeated Admissions of Air. But after each Readmission of fresh Air, the Quantity absorbed was less and less, till no more was absorbed. And it was the same after standing several Weeks, provided in the mean time, too much fresh Air had not been admitted.

3. Antimony and Spirit of Nitre at first absorbed a little Air, it had not much Effect the first Day, but the next Morning I found it generating Air at a good rate, with redish Fumes. I then lifted the inverted Glass *ay* from off the Bolt-head, and immediately immersed its Mouth in Water, in the Vessel *xx*, upon which in an Hour's time, it absorbed four
Inches

Inches Depth of Air; a Quantity equal to one fourth of the whole Capacity of the Glass Vessel *ay*, and it did the same a second, third and fourth time; on the fifth Admission of fresh Air, it absorbed but $3\frac{1}{4}$ Inches Depth, but on the sixth Admission no more was absorbed, nor was the Air turbid.

4. The Bolt-head *b* with its contained effervescent Mass, I placed under a fresh Glass, upon which it absorbed of this fresh included Air, faster than the effervescent Mixture generated Air, so that the Water rose in the inverted Glass; but after some time, the Water changed to a subsiding State, an Argument that more Air was then generated than absorbed.

5. This Air instantly extinguishes a Candle, as do most of the Airs which are tainted with the Fumes of these effervescent Mixtures.

6. Spirit of Nitre with an equal Quantity of Water poured on Filings of Steel, absorbed in an Hour a good Quantity of Air: Three Hours after, when the Air in the Glass *ay* was clear, I let in a like Quantity of fresh Air, but this did not make the Air in the Glass turbid, nor was any of the fresh Air absorbed. Yet when at another time I let the like Mix-

ture of Water and Spirit of Nitre stand six or seven Days, and then admitted fresh Air, the Air in the Glass *ay* became immediately redish and very turbid, and absorbed Air, as fast as the abovementioned Airs of Spirit of Nitre or *Aqua Regia* and Antimony; but it would not do in any considerable Degree a second time.

7. When fresh Air is let into the Receiver *ay*, whose included Air is impregnated with the Fumes arising from the Mixture of compound *Aqua Fortis* or Spirit of Nitre, and *Whitstable Pyrites* mentioned in the following Experiment, then the Air in the Receiver turns very red and turbid, and much Air is absorbed after several repeated Admissions.

8. When fresh Air is thus admitted into the Glasses full of sulphureous tho' clear Air, a good part of the Particles of the fresh Air must needs be reduced, by the sulphureous ones from an elastic to a fixed State, as in the Effervescences of other Liquors. Therefore the rising of the Water in the glass Vessel *ay*, *Fig. 34.* does not seem to be wholly owing to the rebating of the Air's Elasticity in some Degree, but rather to the Reduction of it from an elastic to a fixed State; which is further

ther probable from hence, *viz.* that the whole Quantity of Air admitted at several times, is equal or nearly equal to the Quantity of sulphureous Air *a z*, so that both Airs are at the same time contained within the Space *a z*.

9. When Mercury is dissolved in compound *Aqua Fortis*, the Vapour arising from that Solution will also absorb fresh Air.

10. The several Airs tainted with the Fumes of Vinegar and Oyster-shell, Oil of Vitriol and Oyster-shell, Belemnites and Vinegar, did not absorb Air: But the Air of Belemnites and Spirit of Nitre did absorb some fresh admitted Air, as did the Airs distilled from Tartar and *Newcastle* Coal; but Air distilled from an Ox's Tooth did not absorb any.

11. Since we have under this Experiment so many Instances of the brisk Agitation and Effervescence, which arises from the Mixture of fresh Air, with Air which is strongly impregnated with sulphureous Fumes, which are raised by Effervescence from several mineral Substances; and since we have also from other Experiments many Proofs of the brisk Action and Reaction, there is between elastic Air and sulphureous Particles, may we not with good Reason conclude, that the irksome
Heat

Heat which we feel in what is called a close sultry Temperature of Air, is occasioned by the intestine Motion, between the Air and the sulphureous mineral Vapours which are exhaled from the Earth? Which Effervescence ceases as soon as these Vapours are equably and uniformly mixed with the Air, as happens also in the Effervescences of other Liquors: Thus it is found that all other Fluids and even Metals in Fusion do equally mix and diffuse their several component Parts. The common Observation therefore that Lightening cools the Air seems to be founded on good Reason, that being the utmost and last Effort of this Effervescence.

12. May we not also with some Probability of Reason hence conjecture, that the first ignition or kindling of Lightening is effected by the sudden Mixture of the pure serene Air above the Clouds, with the sulphureous Vapours which are sometimes raised in Plenty, immediately below them; the most dreadful Thunders being usually when the Air is very black with Clouds, it rarely thundering without Clouds? The Clouds serving in this Case, like the inverted Glass *a z*, *Fig. 34.* as a Partition between the pure and sulphureous Airs, which must therefore, upon their sudden Admixture,

mixture, thro' the Interstices of the Clouds, make like the two Airs in the Glass, a more violent Effervescence, than if those Airs had without the Intervention of Clouds more gradually intermixed, by the constant regular Ascent of the warmer sulphureous Vapours from the Earth, and Descent of the cold serene Air from above. And tho' there was no luminous Flash of Light in the Glasses, yet when such sudden Effervescence arises, among a vast Quantity of such Vapours, in the open Expanse of Air, it may not improbably acquire so rapid a Velocity, as to kindle the sulphureous Vapours, and thereby become luminous.

13. And since from the Effects that Lightning is observed to have on the Lungs of Animals which it kills, by destroying the Air's Elasticity in them, as also from its bursting Windows outwards, by destroying the Air's Elasticity on the Outside of those Windows; since it is hence probable that its sulphureous Fumes do destroy a great Quantity of the Air's Elasticity wherever it passes; it should therefore cause the greatest Commotions and Concussions in the Air, when the Air rushes into those thus evacuated Places: Which it must necessarily do with great Velocity. Dr. *Papin* has

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calculated

calculated the Velocity with which Air rushes into an exhausted Receiver, when driven by the whole Pressure of the Atmosphere, to be at the rate of one thousand three hundred and five Feet in a Second of time, *Lowthorp's Abridgment, Philos. Trans. Vol. I. p. 586.* A Velocity something greater than that of Sounds, which is according to Mr. *Derham* at the rate of twelve hundred and eighty Feet in a second, or a Mile in $4\frac{1}{8}$ th Seconds; no wonder then that such violent Commotions should produce Whirlwinds, Hurricanes and Thunder-Showers, especially in the warmer Climates, where both the sulphureous and watry Vapours, being raised much higher and in greater Plenty, cause more violent Effects. These Thunderings are also observed to be frequent at the End of long Frosts, especially far North, and after long Drought, the sulphureous Vapours then arising plentifully out of the Earth.

14. If the first enkindling of Lightening were from burning *Focus's* of the Sun's Rays only, then there could be no Thunder Storms in the Night time, when the Sun was set, but only in the Day time, when it is above the Horizon; yet it is well known that Lightening
and

and Thunder are frequent in the Night, which should therefore at that time arise from an Effervescence in the Air ; which Effervescence may very probably be frequently kindled into Lightning, as the learned Dr. *Boerhaave* observes in his excellent and elaborate *Elementa Chæmiæ*, Vol. I. p. 232. by the Refractions and Reflections of the Sun Beams among the Clouds.

15. That the crooked Dartings of Lightning are not owing to its following a Train of sulphureous Vapours in the Air, is probable hence, viz. that if a large Viol, which has all its Air exhausted, be struck on its Side with the Palm of the Hand, there will issue thence pale, livid, pointed and crooked Flashes, four, five or six Inches long, which Crookedness cannot in this Case be attributed to a Train of sulphureous Vapours; for the same Event will happen, let the Viol be struck never so often, whether it continues in the same Place or be removed. The *Impetus* therefore of Lightning seems to be given it, at or near the first Explosion of the sulphureous Vapours, its Force being more or less in Proportion to the Quantity of those Vapours which are at once enkindled.

EXPERIMENT IV.

1. **I**T was found in *Experiment* XCVI. *Vol.* I. *p.* 224. that the *Walton Pyrites*, which is a vitriolic Stone, absorbed more Air than it generated, with compound *Aqua-fortis* alone, and generated more than it absorbed, when diluted with an equal Quantity of Water. But it is not the same with the vitriolic *Pyrites* which is gathered on the Sea Shore near *Whitstable* in *Kent*, of which Stones green Vitriol or Coperas are there made; for when on five hundred and twenty five Grains or half a cubic Inch of it powdered, a cubic Inch of compound *Aqua-fortis* was poured, it expanded with a red Fume into a Space equal to two hundred and sixteen cubic Inches, but in two Hours time that Space was wholly contracted, and a hundred and eight cubic Inches of Air were absorbed. And the Fumes of this *Pyrites* were of so strongly absorbing a Quality, that when the *Aqua-fortis* was diluted with thrice its Quantity of Water, its Fumes absorbed a hundred and forty four cubic Inches of Air more than was generated.

2. That these Fumes do at the same time both generate and absorb Air, is further evident from the following Experiment, *viz.* I placed at once under a large inverted chymical Receiver, whose Orifice was immersed in Water, five deep and wide glass Tubes which were closed at their lower ends, and bound fast together, and supported at a due Height by a Stick in the midst of them. There was in each of the Tubes a cubic Inch of compound *Aqua-fortis*, on which I let fall at two Hours Distance from each other, the powdered *Whitstable Pyrites*. Upon the two first Mixtures more Air was absorbed than generated; but the Effervescences of the three following Mixtures generated severally more than was absorbed, *viz.* because as appears in several other Instances, when the Air in the inverted Receiver was much impregnated with the sulphureous Fumes of the two first effervescent Mixtures, the Fumes of the three following Mixtures absorbing no more Air, the Quantity generated by those Mixtures, was seen by the subsiding of the Water in the inverted Receiver.

3. This Coperas Stone with Oil of Vitriol or Oil of Sulphur, or Spirit of Salt, each

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diluted

diluted with Water, caused a great Heat, but no visible Effervescence or Fume.

4. In these and the like Experiments I frequently made use of an inverted chymical Receiver instead of the cylindrical Glass, *Fig. 34.* which Receiver was suspended by a Cord, which was braced round it: I first poured the acid Spirit into the Bolthead or large Tube, and then put into the Top of the Bolthead an inverted Neck of a *Florence* Flask which formed a little Tunnel, whose lower Orifice being slightly stopped with a Cork or Plug of Cotten or Flax, I filled the Neck with the Powder, and at the same time put into it a strong Piece of Wire, which was two or three Inches longer than the Neck; then placing the Bolthead or large Tube under the inverted Receiver, I drew up the Water into the Receiver to a proper Height by means of a Syphon: Then with my Hand I raised up the Bolthead or large Tube, so as to make the Wire bear hard against the Top of the inverted Receiver, whereby the Cork being pushed out of the Neck of the Flask, the Powder fell down into the Spirit.

5. But when the five Tubes abovementioned were placed at the same time under the
 6 Receiver,

Receiver, then I tied long Pieces of Pack-thread to the Top of each Wire, whereby pulling only one of those at a time, the Powder fell then only into one Tube. I find these very ready ways to make these Experiments.

E X P E R I M E N T V.

AS this wonderful and important Fluid the Air is found to be very operative in every part of Nature, whether animal, vegetable or mineral, so we cannot easily find a larger Field, for making Variety of Experiments, or wherein we may reasonably hope to have our Researches rewarded with further and more important Discoveries; particularly in relation to the great Use of it, to the Life and Support both of Animals and Vegetables, when pure and uncontaminated, without which it is so far from being beneficial, that it is noxious to them.

I will here therefore subjoin some Experiments which Mr. Professor *Muskhenbroeck* made, with Variety of effervescent Mixtures both in the open Air and in *Vacuo*; an Account of which he has given in his *Addita-
menta ad Tentamina Experimententorum natura-*

lium captorum in Academia del Cimento.

“ 1. A Mixture of three Drams of well
 “ rectified Spirit of Wine, and of an equal
 “ Quantity of Wine Vinegar, made no visi-
 “ ble Commotion, but acquired a Heat from
 “ forty four to fifty two Degrees on *Fahren-*
 “ *hey's Thermometer.*

“ 2. The same Mixture in *Vacuo*, made an
 “ instantaneous Ebullition, which soon ceas-
 “ ed, and acquired a Heat from forty four to
 “ forty nine Degrees. The Mercury in the
 “ Air Pump Gage descended two Lines on
 “ account of the elastic Matter generated in
 “ the Receiver, the Capacity of which was
 “ equal to a hundred and forty two *Rynland*
 “ cubic Inches.

“ 3. Half an Ounce of Spirit of Wine,
 “ and a Dram of Spirit of Sea Salt did, when
 “ mixed, without any sensible Commotion,
 “ acquire a Heat from forty six to fifty.

“ 4. The same when mixed in *Vacuo* ac-
 “ quired one Degree of Heat more.

“ 5. *Monsieur Geoffroy in l'Hist. de l'Acad.*
 “ *Roy. Anno 1727.* says, that almost all the
 “ essential Oils of Plants which are prepared
 “ by Distillation, do when mixed with recti-
 “ fy'd Spirit of Wine produce some Degree of
 “ Cold,

“ Cold, which Mr. *Muskhenbroek* found to be
 “ greater when they were mixed in *Vacuo* ;
 “ in the Mixture of Spirit of Wine and Oil
 “ of Fennel, the Degree of Cold was in *Vacuo*
 “ from forty four to forty two Degrees, which
 “ in the open Air was not sensible.

“ 6. When Oil of Caraway and Spirit of
 “ Wine were mixed in *Vacuo*, the Degree of
 “ Cold was from $45 \frac{1}{2}$ to $41 \frac{1}{2}$, which Cold
 “ was but half a Degree in the open Air.

“ 7. Half an Ounce of Oil of Turpen-
 “ tine, and a like Quantity of Spirit of Wine,
 “ did on Mixture acquire Degrees of Cold
 “ from forty five to forty three Degrees, and
 “ a like Mixture in *Vacuo* to forty two De-
 “ grees.

“ 8. On a Mixture of half an Ounce of
 “ Vinegar with a Dram of red coral, there
 “ was a great Effervescence like the boiling
 “ of hot Water, with many Air Bubbles, the
 “ Heat was from forty four to forty six $\frac{1}{2}$,
 “ Degrees.

“ 9. When a Dram of Crabs Eyes was
 “ put into half an Ounce of Vinegar, there
 “ was instantly a great and lasting Effervef-
 “ cence, with much Froth ; the Heat was in-
 “ creased from forty four to forty six Degrees.

“ 10. On

“ 10. On mixing the like Ingredients in
 “ *Vacuo*, there was a very great Effervescence
 “ with much viscid Froth ; but what was ve-
 “ ry remarkable the Thermometer fell from
 “ forty four to forty three Degrees ; and the
 “ Mercury in the Air Pump Gage subsided
 “ four Lines, the Stones were much less dis-
 “ solved than in the foregoing ninth Experi-
 “ ment.

“ 11. There was a visible Effervescence tho’
 “ little Froth, when half an Ounce of Vi-
 “ negar and a Dram of white Chalk were
 “ mixed, the Heat was increased from forty
 “ four to forty five $\frac{1}{4}$ th.

“ 12. When the same Experiment was re-
 “ peated in *Vacuo*, there was a great frothing
 “ Effervescence, but the Thermometer de-
 “ scended from forty four to forty three :
 “ And there was so much elastic Fluid gene-
 “ rated, as made the Mercury in the Gage
 “ fall four Lines.

“ 13. And the Effects were nearly the same,
 “ both in the Air and in *Vacuo*, where Vi-
 “ negar and a blue *Namur* Stone were mixed.

“ 14. Three Drams of Spirit of Sea Salt
 “ being poured on a Dram of Filings of Iron,
 “ there arose a small tho’ sensible Effervescence,
 whereby

“ whereby the Heat was increased from forty seven to fifty seven Degrees.

“ 15. But when the like Mixture was made in *Vacuo*, there was a great and frothy Effervescence, which continued long, and dissolved the Metal much more than in the foregoing Experiment; the Heat increased from forty seven to seventy Degrees; the Mercury in the Gage did not stir.

“ 16. A Dram of Bismuth being thrown into a like Quantity of Spirit of Sea Salt, there arose a vast frothy Effervescence, with white Fumes; the Heat was so great as to rise from forty seven to a hundred and fifteen Degrees. The same being repeated in *Vacuo*, there was a very great frothy Effervescence, with rising Fumes, but the Heat was only from forty seven to ninety four Degrees; the Mercury in the Gage fell four Inches.

“ 17. A Dram of Golden Marchasite being thrown into three Drams of Spirit of Salt, there was no Effervescence, and very little of it dissolved in a Month, the Heat only from forty seven to forty eight $\frac{1}{4}$ Degrees.

“ 18. But

“ 18. But in *Vacuo* there was a remarkable
 “ frothy cold Effervescence, for the Thermo-
 “ meter sunk one Degree: The Mercury in
 “ the Gage did not move, more of the Mar-
 “ chasite was dissolved than in the former Ex-
 “ periment.

“ 19. Three Drams of this Spirit of Salt
 “ with a Dram of red Coral, made a vehe-
 “ ment frothy Effervescence; its Heat was
 “ from forty seven to fifty six Degrees.

“ 20. The like Mixture in *Vacuo* made a
 “ vast frothing Effervescence, with the same
 “ Degree of Heat as before: The Mercury in
 “ the Gage fell $3\frac{1}{12}$ th Inches.

“ 21. A like Quantity of Spirit of Sea
 “ Salt, being poured on a Dram of pounded
 “ Marble, there was a great frothy continued
 “ Effervescence, the Heat of which was from
 “ forty seven to fifty seven Degrees.

“ 22. The like Mixture in *Vacuo* made a
 “ vast Effervescence, which was soon over;
 “ but its Heat was only from forty seven to
 “ fifty two Degrees. The Mercury in the
 “ Gage descended $3\frac{1}{4}$ Inches, by reason of the
 “ elastic Matter which was generated in the
 “ Receiver.

“ 23. A Dram of Ox Bone being put into
 “ three of Spirit of Salt, raised a great frothy
 “ Effervescence of some Continuance; the
 “ Heat of it was from forty seven to fifty
 “ seven.

“ 24. The same being repeated in *Vacuo*,
 “ made a greater Effervescence, tho’ not of
 “ so long Duration; the Heat rising only to
 “ fifty five Degrees.

“ 24. Spirit of Nitre with Rain Water
 “ causes a Heat from forty five to fifty three
 “ Degrees, with an equal Quantity of distill-
 “ ed Elder Water from forty seven to fifty one
 “ Degrees. The same Mixture in *Vacuo* caus-
 “ ed a visible Effervescence, with some Vapour,
 “ the Heat was from forty one to fifty five
 “ Degrees.

“ 25. Scurvy-grass Water mixed with an
 “ equal Quantity of Spirit of Nitre, caused
 “ an immediate short Perturbation, which
 “ raised a Heat from forty six $\frac{1}{2}$ to fifty five
 “ Degrees.

“ 26. The same being repeated in *Vacuo*,
 “ there was a sort of Effervescence with Va-
 “ pours, the Heat was also fifty five Degrees.

“ 27. A Dram of *Ceruse* with Spirit of
 “ Nitre made a great and hot Effervescence,

“ viz.

“ viz. from forty six to fifty eight Degrees.

“ 28. But the same Mixture in *Vacuo*
 “ made a considerable Ebullition with Froth,
 “ its Heat was from forty six to seventy two
 “ Degrees. The Mercury in the Gage did
 “ not stir.

“ 29. A Dram of *Saccharum Saturni* with
 “ Spirit of Nitre, caused no sensible Commo-
 “ tion, yet the Heat rose from forty six to
 “ fifty two Degrees.

“ 30. But the same Mixture in *Vacuo*, made
 “ a considerable short Effervescence with Froth,
 “ the Heat rose from forty six to fifty four
 “ Degrees.

31. A Dram of *Minium*, thrown into Spi-
 “ rit of Nitre, made a small, but visible Effer-
 “ vescence, almost without Froth or Vapour.

“ 32. But the like Mixture in *Vacuo* made
 “ a remarkable Effervescence with Froth, of
 “ long Continuance, and ten times greater
 “ than before the Heat increasing from forty
 “ six to eighty eight Degrees.

“ 33. A Dram of *Lytharge* in Spirit of Ni-
 “ tre, made a considerable Effervescence with
 “ Froth, which soon ceased; the Heat in-
 “ creased from forty six $\frac{1}{2}$ to sixty two De-
 “ grees.

“ 34. The

“ 34. The like Mixture in *Vacuo*, made
 “ an Effervescence of long Continuance, its
 “ Heat increasing from forty six $\frac{1}{2}$ to sixty
 “ Degrees.

“ 35. A Dram of Tin dropped into Spirit
 “ of Nitre, did in an instant make a most sur-
 “ prizingly violent Effervescence, so that the
 “ Heat increased from forty six $\frac{1}{2}$ to two hun-
 “ dred and fifty; the Fumes were so great as
 “ to fill the whole House, and fume out at
 “ the Door: The whole Tin was instantly
 “ turned into a white dry Powder, resembling
 “ true Calx of Tin. This Experiment ought
 “ to be made with great Caution, lest the
 “ Lungs be injured thereby. When *Aqua-*
 “ *fortis* and Tin were mixed the Heat was
 “ only from forty six to a hundred and sixty
 “ three Degrees.

“ 36. When a Dram of Tin was dropped
 “ into Spirit of Nitre in *Vacuo*, there was
 “ also a violent Effervescence, tho’ not near so
 “ great as the other, the Heat arose from forty
 “ six $\frac{1}{2}$ to a hundred and eighty Degrees. Some
 “ of the volatile Fumes were elastic, for the
 “ Mercury in the Gage fell two $\frac{1}{3}$ Inches.

“ 37. Filings of Iron and Spirit of Nitre
 “ made a vast frothy Effervescence, with large
 “ yellow

“ yellow fetid Fumes, the Heat rose from forty six to a hundred and forty five Degrees.

“ 38. The like Mixture in *Vacuo*, boiled much, with thick yellow Fumes, its Heat was from forty six to a hundred and twenty Degrees: The Mercury in the Gage descended four $\frac{1}{2}$ Inches; with fuming Spirit of Nitre the Heat is so sudden and great as to break the Thermometers.

“ 39. With a Dram of Filings of red Copper and Spirit of Nitre, the Effervescence with yellow Fumes was great, the Heat from forty six to a hundred and six; a small part of the Copper being dissolved, it made a fine green Tincture.

“ 40. The like Mixture in *Vacuo* made a great Effervescence, the Heat being from forty six to a hundred Degrees: The Mercury in the Gage descended three $\frac{1}{2}$ Inches.

“ 41. Spirit of Nitre and a Dram of Brass made a vast Effervescence, with plentiful red hot Fumes, the Heat thence arose from forty eight to a hundred and eighty Degrees. All the Metal was dissolved, affording a beautiful green Tincture.

“ 42. From the like Mixture in *Vacuo*, arose a very great Effervescence with plentiful

“ tiful Fumes, the Heat increasing from forty
 “ eight to a hundred; all the Metal was like-
 “ wise presently dissolved into a Tincture like
 “ the former. So that the Effect of Spirit of
 “ Nitre is in *vacuo* the same on Brass and
 “ Copper, but very different in the open
 “ Air. The Mercury in the Gage subsided
 “ $3 \frac{1}{12}$ Inches.

“ 43. A Dram of Filings of Silver and
 “ Spirit of Nitre did effervesce, but not much,
 “ some Fume flew off; the Heat increased
 “ from forty eight to fifty seven.

“ 44. The like Mixture in *vacuo* made an
 “ Ebullition, like boiling Water, but with
 “ little Froth; but, what is wonderful, there
 “ was no Heat excited the Thermometer
 “ at forty eight Degrees.

“ 45. Spirit of Nitre poured on a Dram of
 “ Bismuth, made an inexpressibly violent Ef-
 “ fervescence, and such plentiful Fumes as
 “ filled the whole House like those from
 “ Tin: The Heat increased from forty eight
 “ to two hundred and forty three. When
 “ the Ebullition ceased the Mixture fell into
 “ a dry yellowish *Calx*.

“ 46. The like Mixture in *vacuo*, made
 “ a vast Effervescence with plentiful Fumes,

“ which like Dew ran down the Sides of
 “ the Receiver; the Heat increased to a
 “ hundred and fifty. The Mercury in the
 “ Gage descended $2 \frac{2}{3}$ Inches; less of this
 “ Metal was reduced to a *Calx*, than in the
 foregoing Experiment.

“ 47. A Dram of Golden *Marchasite*
 “ dropped into Spirit of Nitre, made a great
 “ frothing Ebullition, with plenty of thick
 “ yellow Fumes, it was almost all dissolved.

“ 48. A Dram of crude *Antimony*, with
 “ Spirit of Nitre, made an Ebullition, like
 “ that of boiling Water, with some Fumes,
 “ the Heat rose from forty six to seventy three
 “ Degrees; the greatest part of it was undissolved.

“ 49. The like Mixture in *vacuo* made a
 “ considerable Ebullition with Froth, and
 “ plentiful Fumes; the Heat was also from
 “ forty six to seventy three, less of this was
 “ dissolved than of that in the open Air. The
 “ Mercury in the Gage descended $2 \frac{1}{2}$ Inches.

“ 50. A Dram of the *Lapis Calaminaris*
 “ mixed with Spirit of Nitre, caused a visible
 “ Emotion, and the Heat was from forty
 “ six to sixty Degrees.

“ 51. The

“ 51. The like Mixture in *vacuo* made a
 “ very remarkable Ebullition, with plentiful
 “ Fumes, which covered the Sides of the
 “ Receiver ; the Heat being increased from
 “ forty six to a hundred and two Degrees.

“ 52. A Dram of *Tutti* mixed with Spirit
 “ of Nitre, caused no visible Motion, the
 “ Heat was increased from forty six to sixty
 “ nine Degrees.

“ 53. The like Mixture in *vacuo* made a
 “ remarkable frothy Effervescence, with a
 “ Heat from forty six to eighty Degrees ;
 “ more of it was dissolved than in the fore-
 “ going Experiment. The Mercury in the
 “ Gage descended two Lines.

“ 54. A Lixivium of calcined Ashes, with
 “ an equal Quantity of Spirit of Nitre, made
 “ a vehement Effervescence, with Froth and
 “ Plenty of Fumes, the Heat being increas-
 “ ed from $46\frac{1}{2}$ to eighty five Degrees.

“ 55. A like Mixture in *vacuo* made a
 greater Effervescence, tho’ its Heat was less,
 “ viz. from $46\frac{1}{2}$ to seventy four Degrees.
 “ The Mercury in the Gage fell seven Inches.

“ 56. Spirit of Nitre mixed with an equal
 “ Quantity of new Milk, made no sensible
 “ Emotion, but yet the Heat increased from
 “ 47 to 55^2 Degrees. X 2 “ 57. When

“ 57. When Spirit of *Sal Ammoniac* and
 “ Spirit of Nitre were three Drams of each
 “ mixed, they made some Ebullition, and the
 “ Heat rose from forty seven to eighty three
 “ Degrees; the Mixture continued limpid and
 “ colourless.

“ 58. When these two Liquors were plac-
 “ ed in distinct Vessels under an Air Pump
 “ Receiver, they both fumed while the Air
 “ was exhausting and after it was all exhaust-
 “ ed. As soon as the Spirit of Nitre was
 “ poured on the Spirit of *Sal Ammoniac*,
 “ there rose an instantaneous Motion, which
 “ like exploding Gun-powder, dispersed part
 “ of the Liquor; but when these Spirits were
 “ afterwards mixed more gradually, then the
 “ following Explosions were less violent, the
 “ Heat increased from forty seven to sixty
 “ three Degrees. The Mercury in the Gage
 “ descended four Inches.

“ 59. Tho’ when equal Quantities of fresh
 “ Urine and Spirit of Nitre were mixed,
 “ there was no sensible Effervescence, yet the
 “ Heat increased from forty seven to fifty two
 “ Degrees.

“ 60. The same Mixture in *vacuo* had no
 “ visible Commotion, yet the Heat was raised
 “ from

“ from forty seven to fifty seven Degrees.

“ 61. Spirit of Nitre, mixed with an equal
“ Quantity of Spirit of Vinegar, there was
“ scarce any visible Commotion, yet the Heat
“ arose from forty six to fifty four Degrees.

“ 62. The same Mixture in *vacuo* had a
“ small Commotion during its Mixture, and
“ acquired a Heat from forty six to fifty six
“ Degrees. The Mercury in the Gage did
“ not stir.

“ 63. Spirit of Nitre, with half a Dram of
“ Crabs Eyes mixed, made a considerable
“ frothing Effervescence, whereby the Heat
“ was increased from forty six to fifty four
“ Degrees.

“ 64. The same Mixture did in *vacuo* make
“ a great frothing Effervescence, four times
“ greater than the former, its Heat was rais-
“ ed from forty six to fifty six Degrees, they
“ were wholly dissolved in both Cases.

“ 65. Equal Quantities of Lemon-juice
“ and Spirit of Nitre, caused no visible Emo-
“ tion when mixed, and tho' the Spirit of
“ Nitre sunk instantly to the Bottom, the
“ Lemon-juice swimming on the top, yet the
“ Heat increased from forty six to $52\frac{1}{2}$ De-
“ grees.

“ 66. The same Mixture in *vacuo*, tho' it
 “ caused no visible Emotion, yet raised a Heat
 “ from forty six to fifty six Degrees, the Mer-
 “ cury in the Gage standing still.

“ 67. Equal Quantities of Spirit of Nitre
 “ and *French* White Wine, when mixed,
 “ caused a Heat from forty six to fifty three
 “ Degrees, yet there was no visible Emotion
 “ in it.

“ 68. Equal Quantities of Spirit of Nitre
 “ and distilled Oil of Sassafras Wood, made
 “ a violent Effervescence, with Fume and
 “ Heat. Yet Spirit of Nitre and two Drams
 “ of Oil of Anniseed, when mixed, neither
 “ made any Emotion, nor Alteration in the
 “ Heat.

“ 69. *N. B.* This Spirit of Nitre was made
 “ with Bole, it emits few Air Bubbles in *va-*
 “ *cuo*; whereas the fuming Spirit of Nitre,
 “ and Spirit of Sea Salt abound with Plenty
 “ of Air; hence before these are mixed in
 “ *vacuo*, it is requisite to wait for some time
 “ till they are freed from their Air, that we
 “ be not deceived by taking Air Bubbles for
 “ Effervescences.

“ 70. *Monsieur Geoffroy's* fuming Spirit of
 “ Nitre, when mixed with Oil of Turpentine
 “ or

“ or other essential Oils of Plants, does in-
 “ stantly kindle into a great Flame ; this Spi-
 “ rit is made by distilling in a reverberatory
 “ Heat two Pounds of Nitre with one of Oil
 “ of Vitriol.

“ 71. Twenty Drops of this Spirit being
 “ mixed in *vacuo* with a like Quantity of
 “ Oil of Carraways ; they made a great Ef-
 “ fervesence without Flame, and the Heat
 “ was raised to two hundred and sixteen De-
 “ grees : When the intestine Motion seemed
 “ to be over, he let Air into the Receiver, on
 “ which there was a Flame raised, which
 “ was soon extinguished, by the great Fumes,
 “ for want of a free Air. Neither did Oil
 “ of Turpentine, Oil of Rosemary, or Oil of
 “ Anniseed flame in *vacuo*, nor when Air was
 “ let in, till a little Oil of Vitriol was added,
 “ which yet did not cause the Oil of Anniseed
 “ to flame.

“ 72. Three Drams of Oil of Vitriol being
 “ mixed with an equal Quantity of Rain
 “ Water, there was no visible Emotion, yet
 “ the Heat increased from forty eight to nine-
 “ ty two Degrees.

“ 73. Three Drams of Oil of Vitriol, and
 “ a like Quantity of the distilled Water of

“ Scurvy-grafs, being mixed, made no Emo-
 “ tion, but the Heat was greater than with
 “ Rain Water, *viz.* from forty eight to nine-
 “ ty two Degrees.

“ 74. Like Quantities of Oil of Vitriol
 “ and distilled Elder Water caused on Mix-
 “ ture a Heat from forty eight to seventy
 “ Degrees. Whence he observes that Elder
 “ Water, which when drank refrigerates the
 “ Body, has some Property in it, which
 “ checks Heat to twenty two Degrees less
 “ than Rain Water; whereas Scurvy-grafs
 “ Water which heats the Body, has some-
 “ thing in it which with Oil of Vitriol causes
 “ by six Degrees a greater Heat than Rain
 “ Water.

“ 75. Three Drams of Oil of Vitriol and
 “ a like Quantity of Rhenish Wine, when
 “ mixed, caused a Heat from fifty nine to
 “ $99 \frac{1}{2}$ Degrees. He further observed that
 “ when the Proportion of Wine in the Mix-
 “ ture was either greater or less, that the
 “ Heat was in both Cases less.

“ 76. Two Drams of *Sal Ammoniac* be-
 “ ing thrown into three Drams of Oil of Vi-
 “ triol, there was instantly a great frothing
 “ Effervescence, which filled the whole Place
 “ with

“ with acrid Fumes : Which Fumes were so
 “ warm that they raised the Thermometer,
 “ which hung over them, ten Degrees ;
 “ while at the same time another Thermo-
 “ meter which was placed in the Mixture,
 “ descended by reason of the increased Cold
 “ from sixty to forty eight Degrees ; the
 “ greatest part of the Salt was dissolved. If
 “ while the Effervescence continues, some
 “ Water be poured in, the Thermometer,
 “ which was in a descending State on ac-
 “ count of the increasing Cold, will imme-
 “ diately ascend on account of the then much
 “ increased Heat.

“ 77. He repeated this remarkable Expe-
 “ riment in *vacuo* in the following manner,
 “ viz. He suspended in the Receiver one
 “ Thermometer, so as to be above the Froth
 “ of the effervescent Mixture four or five
 “ Lines of an Inch ; the other Thermometer
 “ he put into the Vessel in which was a
 “ Dram of *Sal Ammoniac*, suspending over
 “ it in a moveable Viol, three Drams of
 “ Oil of Vitriol ; then having drawn the
 “ Air carefully out of the Receiver, he let
 “ all stand thus for an Hour, that they might
 “ have an equal Heat ; after which on pour-
 “ ing

“ ing the Oil of Vitriol on the *Sal Ammo-*
 “ *niac*, there was instantly a great Effer-
 “ vescence, with such Plenty of ascending
 “ Fumes, as filled the Receiver, so that he
 “ could scarce distinguish the Degrees on
 “ the Thermometers, but in half a Minute
 “ the Receiver was more pellucid ; the Li-
 “ quor in the Thermometer which was pla-
 “ ced in the effervescent Mixture, had de-
 “ scended from sixty seven to forty six De-
 “ grees in a Minute’s time ; after which it
 “ began to ascend, and when it had come
 “ to fifty eight, then the Liquor in the o-
 “ ther Thermometer was at sixty nine : and
 “ when the lower Thermometer was at sixty,
 “ then the upper one was at sixty nine one
 “ Quarter. But when after two Minutes the
 “ lower Thermometer was sixty eight, then
 “ the upper was seventy ; after another Mi-
 “ nute both Thermometers were at seventy :
 “ But after five Minutes the lower Thermo-
 “ meter was at seventy two, the upper one
 “ continuing at seventy. And the lower
 “ one ascended to seventy four, even after a
 “ Quarter of an Hour ; and when the Effer-
 “ vescence ceased, yet the upper one con-
 “ tinued at seventy, the Effervescence lasted

“ at least twenty Minutes. For greater Cer-
 “ tainty, he repeated this Experiment twice
 “ and found the Event the same. So the
 “ ascending Vapours did in *vacuo* acquire
 “ three Degrees of Heat, the refrigerating
 “ Mixture twenty one Degrees of Cold; it
 “ at first increased in Degrees of Cold, but
 “ afterwards acquired Heat, as the Mixture
 “ abated of its Emotion; for while there
 “ was a great Effervescence so long the Cold
 “ continued. There was a remarkable Dif-
 “ ference between this Experiment in *vacuo*,
 “ and the other in the open Air, where the
 “ ascending Vapour was remarkably warm,
 “ but in *vacuo* there was no Warmth given
 “ to the upper Thermometer till after the
 “ Effervescence was over, when the Fumes
 “ did not ascend.” Whence I suspect that
 the Heat which the upper Thermometer
 then acquired, might be owing to the Heat
 communicated to it, from the Mixture be-
 low, which being at a Distance from it could
 not communicate its whole Heat, *viz.* seventy
 four Degrees: We may hence also conclude
 that in the open Air, the Action and Re-
 action of the Air, greatly increased the Effer-
 vescence, and consequently the Heat of the
 Fumes.

“ 78. The

“ 78. The ingenious Author observes hence
 “ the great Variety of very different Effects,
 “ which those several effervescent Mixtures
 “ produce, *viz.*

“ 79. As first, that the Effervescences of
 “ the same Bodies produce the same Heat
 “ in the open Air and in *vacuo*, as appears
 “ by Number 48 and 49, when Crude An-
 “ timony and Spirit of Nitre were mixed
 “ together.

“ 80. Sometimes Effervescences are greater
 “ and hotter in the Air than in *vacuo*.
 “ Thus Number 16. Bismuth and Spirit of
 “ Salt made a greater Effervescence in the
 “ open Air, and acquired a Heat from forty
 “ seven to a hundred and fifteen Degrees,
 “ whereas in *vacuo* the Heat was raised
 “ only from forty seven to ninety four De-
 “ grees. It was the same also in Number
 “ 33 and 34, in 35 and 36, in 37 and 38, in
 “ forty five and forty six.

“ 81. On the contrary the Effervescences
 “ are sometimes hotter in *vacuo* than in the
 “ open Air, as is evident by comparing Num-
 “ ber 14 and 15, where Spirit of Sea Salt
 “ and Filings of Iron made a more vehement
 “ Effervescence in *vacuo* than in the Air; for
 “ in

“ in *vacuo* the Heat increased from forty
 “ seven to seventy, but in the Air, only from
 “ forty seven to fifty seven Degrees. It was
 “ the same in Number 24, in 27 and 28, in
 “ 31 and 32, in 50 and 51, in 52 and 53, in
 “ 63 and 64.

“ 82. With some Mixtures, though there
 “ be no visible Effervescences in the open
 “ Air, there are great ones in *vacuo*, as in
 “ Number 1 and 2, in 50 and 51, in 52
 “ and 53.

“ 83. There are Effervescences which are
 “ hot in the Air, but acquire no Heat in
 “ *vacuo*, as was the Event in Number 43
 “ and 44.

“ 84. There are some Effervescences which
 “ produce a greater Degree of Cold in *va-*
 “ *cuo* than in the open Air, as in the Mix-
 “ ture of Spirit of Wine and Oil of Fennel
 “ Number 5.

“ 85. There are others which produce
 “ Heat in the open Air, but Cold in *vacuo*,
 “ as in the Mixture of Crabs-Eyes and Vi-
 “ negar Number 9, 10.

“ 86. There is sometimes a great Heat ac-
 “ quired without a sensible intestine Motion,
 “ as on the Mixture of Oil of Vitriol and
 “ Water, Number 72, 73, 74. “ 87.

“ 87. There are Effervescences which ac-
 “ quire neither Heat nor Cold, as Spirit of
 “ Sea Salt with Lead in *vacuo*.

“ 88. There are also great Effervescences
 “ which produce Cold, as Oil of Vitriol and
 “ *Sal Ammoniac*, as in Number 76, 77 ; it
 “ is the same also with Oil of Vitriol and
 “ volatile Salt of Urine.

89. Whence our Author would infer, that
 these Degrees of Cold are acquired by the
 flying away of the Fire with the rising
 Fumes ; and that the Heat of these Effer-
 vescences is not owing to the intestine Motion
 of the Parts, but to a real inherent elemental
 Fire, on the flying away of which Cold is
 produced.

90. But when we consider, how vastly
 great the attractive and repulsive Force of
 many of the Particles of Matter are, near
 the Point of Contact, it seems not impro-
 bable that the acquired Heat of effervescent
 Mixtures may be owing to the intestine Mo-
 tion arising from those contranitent and much
 agitating Powers ; which as they are varied
 by infinitely different Combinations, so their
 Effects must needs be proportionably varied,
 some Combinations tending to promote and
 greatly

greatly increase the effervescent Vibrations of the Particles, while others as much retard them: But as we cannot pry into the various Position of these Particles, in their several Combinations, on which their different Effects depend, so it will be difficult to account from any Principle, even though a true one, from the very different Effects of effervescent Mixtures.

“ 91. He further observes, that Bodies are
 “ sometimes dissolved more by effervescent
 “ Menstruums in *vacuo* than in the Air: As
 “ Lead is when thus mixed with Spirit of
 “ Salt. And Filings of Iron mixed in the
 “ same in Number 15. As also *Tutti* with
 “ Spirit of Nitre Number 53.

“ 92. Yet Bodies are often more dissolved
 “ by a Menstruum in the open Air, than in
 “ *vacuo*, as Brass in *Aqua fortis*.

93. He observes also, that there are frequently generated in Effervescences, either in the Air or in *vacuo* elastic Vapours, which are like Air; and are doubtless true permanent Air, for I have kept some of these thus generated Airs by me for six Years; after which, on compressing them in a condensing Engine, in the same manner as is mentioned

oned in *Experiment* LXXVII. Vol. I. p. 191. I found that with equal Weights they were compressed exactly in the same Proportion with common Air, which was included with them in the Condenser. I tried also Air which had been distilled from Rhenish Tartar, the Day before, which I repeated again a Week after, in which time one fourth of the Air of Tartar had lost its Elasticity, as I found by the Degree of rising of the Water up the inverted Tube, in which it was contained. That I might with the greater Certainty be assured of the Degrees of Compressibility of these different Airs, I divided the Capacities of two equal Tubes into quarter cubic Inches, by pouring severally those Quantities of Water into the Tubes, and then cutting Notches with a File, on the sides of the Tubes at the several Surfaces of the Water ; by which means I could see by the ascent of the compressed Water in the Tubes, that both the factitious and common Air were exactly alike compressible in all Degrees of Compressure, from the beginning till they were loaded with a Weight equal to that of three Atmospheres, which was the farthest that I durst venture, for fear of bursting the Glass Receiver.

94. And as to the noxious Quality of these factitious Airs, whether they are made by Fermentation, Effervescence, or Distillation; this cannot reasonably be an Objection against their being true Air, because it is well known that common Air is frequently also impregnated with most noxious and deadly Vapours. Thus the Fumes which ascend up in the Air from fermenting Wines, are very pernicious, those also from burning Brimstone are most deadly; and such Mr. *Hawksbee* found the common Air which passed thro' heated Iron and brass Tubes, but the Air which passed through a heated Glass Tube was not noxious; an Argument that the Vapours which arose from the Iron and Brass were noxious, and not the hot Air. There are many Instances in the abovementioned Experiments of Mr. *Muskhenbroek*, of Plenty of unelastic Vapours arising from the effervescent Mixtures in *vacuo*, most of which Vapours were doubtless very noxious, though they had' no new generated elastic Matter among them, as was evident by the not subsiding of the Mercury in the Gage: Whence it seems probable, that the Noxiousness of new generated Air, either by Fermentation,

Effervescence, or Fire; as also of common Air impregnated with the like Fumes, is principally owing to those Fumes or Vapours, as mentioned in Experiment CXVI. Vol. I. and not to a rebating of the Degrees of its Elasticity, for that we see is exactly the same with that of common Air.

95. It is observable that though there was a very great Effervescence made by many of the abovementioned Mixtures in *vacuo*, yet no elastic Air was generated by several of them, and but little by many others, not near so much as would probably have been made in the Bolt-head Fig. 34. p. 168. Vol. I. where I have given many Instances of much greater Quantities of Air being generated by effervescent Mixtures; the Reason of which Difference seems to be this, *viz.* that the Action and Reaction of the common Air with the effervescent Mixture in the Bolt-head, contributed to throw off more Air into an elastic State, than was done in *vacuo* where there was none of that Air.

EXPERIMENT VI.

1. **I**N Experiment CXVI. Vol. I. p. 264.

I have given an account of the result of several Experiments made by the Respiration of Air, included in Bladders. In order to obviate the Inconvenience that I apprehended might arise from the rancid Vapours of Bladders, I made use of the following Method, to try with greater Ease and Accuracy, how long I could breathe to and fro, a certain Quantity of Air, and to find how much of its Elasticity was in that time destroyed, *viz.*

2. I cemented fast at a Hole in the Top of an Air Pump-Receiver, a wooden Fosse. I set this Receiver, whose Diameter was nine Inches, in a wide Vessel which had two Inches depth of Water in it, leaving room at the Bottom for the Water to pass freely to and fro. In this Position the Quantity of Air in the Receiver was equal to five hundred and twenty two cubic Inches. Then stopping my Nostrils, I first breathed out what Air I could from my Lungs, and then applied my Mouth to the end of the Fosse,

where I breathed to and fro, the five hundred and twenty two cubic Inches of Air, for two Minutes and half; when growing uneasy, I breathed out all the Air I could from my Lungs, as at the first, at the same time giving a Signal to a Stander by, to mark the Height of the Water in the Receiver with a piece of Chalk; and upon measuring I found that eighteen cubic Inches, or $\frac{1}{29}$ th Part of the whole Air, was in that time reduced from an elastic to a fixed State; but some further Allowance is to be made on account of the Expansion of the Air, by reason of the Heat it had received in Respiration.

3. We see in this Experiment that near two Gallons of Air, which received no noxious Fumes from the Receiver, yet being breathed to and fro for two Minutes and half, became thereby unfit for Respiration: Whence no wonder that the Air should be infected, and apt to breed Distempers in close Prisons, where not only the Breath, but also the plentiful Perspiration of many confined together stench the Air, and make it apt to breed what are called Goal Distempers; which Inconvenience might in a good Mea-

sure be prevented, if Goals were so contrived as to have a free Passage for the Wind to blow through them, and thereby communicate fresh Air, for want of which many of those unhappy Persons are not only deprived of Liberty in Goals, but too often even of Life also.

4. I have been informed by a Person long experienced in maritim Affairs, that when the Air between Decks has grown offensive by the Vapours arising from the Bodies of many Persons, that it has been found of some Benefit towards purifying that Air, to wash the Beams of the Decks with Vinegar, and to sprinkle it about; and accordingly in *Experiment* CXVI. Vol. I. p. 266. it was found that an Air which passed through Cloths dipped in Vinegar could be breathed to and fro as long again as the like Quantity of Air which was not thus purified; so that the sprinkling of Vinegar between Decks might a little refresh the Air; yet where the Stench is great it can be of but little Benefit, and that only for a short time; nothing but a thorough ventilating Air can then be an effectual Cure. Vinegar has been long looked on as anti-pestilential; whence it is probable,

that there may be a Ferment between this Acid, and the then too alkaline Air, which may thereby be reduced in some Degree from its alkaline, to a neutral, more wholesome State, for many acid and alkaline Mixtures produce neutral Liquors.

5. Now I found in the following manner, the Quantity of Moisture with which these five hundred and twenty two cubic Inches of Air were impregnated, by being breathed to and fro, *viz.* I broke off the Neck of a *Florence* Flask, so low, that the Diameter of its Orifice was three Quarters of an Inch, and then filled it within an Inch of the Top, with Wood Ashes well burnt : Then I run a Glass Tube down to the Bottom of the Ashes, and covered the Mouth of the Flask with a fine Rag, to prevent the Ashes being blown out, when I breathed through them. Then pinching my Nostrils close, I breathed through the Glass Tube, which conveyed my Breath to the Bottom of the Ashes ; they being very dry and scarce cold, imbibed by their lixivial Salt, the Moisture of my Breath as it passed through them ; and having exactly poised the Flask and Ashes in a Scale, before I began to breathe through them, I

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found

found that after having breathed fifty times through them, the Ashes had increased in Weight seventeen Grains. The Air which I there inspired was very dry, for there were constant Fires in the room ; so that the increase of Weight which the Ashes acquired, must come from the Moisture carried off from my Lungs. And this may be taken to be nearly the Quantity of Moisture with which the abovementioned five hundred and twenty two cubic Inches of Air were impregnated, when it became unfit for Respiration ; for we breathe fifty times in two and a half Minutes. Now a cubic Inch of Water weighing two hundred and fifty four Grains, the five hundred and twenty two cubic Inches will weigh 132588 Grains, whence the like bulk of Air, which is eight hundred times lighter, will weigh 165.7 Grains, of which the abovementioned seventeen Grains of Moisture is but $\frac{1}{98}$ th Part. Now this Proportion of additional Moisture in the Air, does not seem alone sufficient to disqualify it for Respiration : For the common Air has frequently a greater Proportion of Moisture in it, *viz.* a Quantity equal to the one third, and sometimes to one half of

the Weight of the Air, which passed thro' burnt Ashes into an exhausted Receiver, tho' in a dry Summer Air there was no Moisture found by this Means, as Mr. *Peter Van Muskhensbroek*, who made the Experiment, relates in his *Oratio de Methodo instituendi Experimenta Physica*, p. xxviii. *Vide Tentamina Experimentorum naturalium captorum in Academia del Cimento*. We may therefore hence reasonably conclude that these five hundred and twenty two cubic Inches of Air were disqualified for Respiration, not barely by such an additional Moisture, but also from some ill Qualities in that Moisture; I suspect among others, the Grossness of these Exhalations from the Lungs, which may hinder their free entrance into the minute Vesicles; for it is with very great Difficulty, that the almost suffocated Lungs can, in this Experiment, be but a little dilated.

6. We may also from this Experiment on Wood-Ashes, estimate the Quantity of Moisture that is carried off by Respiration; for since a Quantity equal to seventeen Grains was breathed off in fifty Expirations, there will proportionably be four hundred and eight Grains evaporated in the twelve hundred Expirations

pirations of an Hour; and in twenty four Hours 9792 Grains, or 1. 39-Pound; which supposing the Surface of the Lungs to be as found in Vol. I. p. 242. 41635 square Inches, then the Quantity evaporated from that inward Surface will be $\frac{1}{1074}$ th part of an Inch depth.

EXPERIMENT VII.

I. **T**HE Consideration of the continued Succession of fresh Air, which is found necessary to keep a Fire burning; also of the intense Degree of Heat which a Fire soon acquires by being vigorously blown on by Bellows; these Considerations put me upon trying by Experiment what the Velocity and Force of that Air might be; which I found by applying a proper mercurial Gage to the Nose of a Pair of Smith's Bellows; whereby I found the Force of the compressed Wind in the Bellows, was sufficient to raise the Mercury in the Gage about an Inch high, sometimes a little more and sometimes a little less. Whence the Force with which the Bellows impel Air into the Fire, is nearly equal to $\frac{1}{3}$ th Part of the Weight of the Atmosphere,

sphere, which Force does, as we see, make the compressed Air rush out of the Bellows with great Velocity.

2. Which Velocity I estimated in the following manner, *viz.* by measuring the Surface of the upper Board of the Bellows, and also the Space they descended in a Second of time; whence having got the Quantity of Air expelled in that time, *viz.* four hundred and ninety five cubic Inches, this divided by the Area of the Orifice of the Nose of the Bellows gave eight hundred and twenty five Inches, or 68. 73 Feet, the Length of the Cylinder of Air, which rushed out of the Bellows into the Fire in that time. Which Air being compressed with a Weight $\frac{1}{30}$ th Part greater than the Weight of the Atmosphere, its Quantity was $\frac{1}{30}$ th Part of 495 cubic Inches greater, *viz.* 16. 5 Inches, in all 511. 5 cubic Inches of Air; which being thus forcibly impelled into the Fire with a Velocity of 68. 73 Feet in a Second of time, the Action and Reaction between the elastic vibrating Matter of the Fewel, and that of the Air, being thereby so greatly increased, as to cause a most intense Heat, sufficient to melt Metals.

The

The Force with which the Bellows of an Organ impel Air into its Pipes, being taken in this manner, might not the Velocities of the Undulations of Air, which are requisite to form those Sounds, be thence estimated? The Velocity of undulating Air to that of Water is as eight hundred sixty five to one, *viz.* nearly as their specific Gravities.

A
DESCRIPTION
OF A
SEA-GAGE,

Wherewith to Measure unfathomable
Depths of the Sea.

1. **I**N my first Volume under *Experiment* LXXXIX. p. 209, I proposed a Method for finding out the Depth of the Sea at unfathomable Depths; which Method the ingenious Dr. *Desaguliers* put in practice before the Royal Society, with a Machine which he contrived; the Description of which he has given in the *Philosophical Transactions*, Number 405. I shall here more particularly describe how to prepare and graduate this Sea-Gage.

2. Suppose

2. Suppose there be an Iron or Copper Tube, or a Musket Barrel of any Length, as fifty Inches, having its upper end well closed up : If this Tube be let down, in this Position, about thirty three Feet in the Sea, for a Column of Sea Water of that Height is nearly equal to the middle Weight of our Atmosphere ; then consequently from a known Property of the Air's Elasticity, it will be compressed into half the Space it took up before, so that the Water will ascend half way up the Tube, and if the Tube be let down thirty three Feet deeper, the Air will be compressed into one Third of its first Dimension, and so on $\frac{1}{4}$ th, $\frac{1}{5}$ th, $\frac{1}{6}$ th, &c. The Air being constantly compressible in Proportion to the incumbent Weight ; whence by knowing to what Height the Water has ascended in the Tube, we may readily know to what Depth the Tube has descended in the Sea.

3. Now to measure the Depth of one of these Columns of Sea Water, whose specific Gravity to pump Water is found to be as forty one to forty. First by a Line let the Iron Tube, with a Weight at its Bottom, sink about thirty three Feet, which Depth in
Salt

Salt Water will nearly answer to the Weight of the Air, at a mean Height of the Barometer, then draw up the Tube and observe how far the Water rose ; if thirty three Feet of Water is equal to one Atmosphere, then will the Water rise so high as to fill exactly one half of the Tube : But if the Water rises higher or lower than half way, then by the Rule of Three say, as the Number to which the Water rises is to one, so is thirty three to the Number of Feet, measuring the Depth of the Column required. For Example, suppose the Water rises (when the Tube is let down thirty three Feet) only nine Tenths of half way, then say as $9 : 10 :: 33 : 36\frac{1}{2}$ Feet the Depth of each Column, which being once known, the Number of Columns of Water is to be multiplied by this Number of Feet, whereby the Depth of the Sea in Feet will be known.

4. But since when the Instrument has descended to the Depth of ninety nine Columns, or ninety nine times thirty three Feet, the Air will be compressed into the $\frac{1}{100}$ th Part of fifty Inches, that is, half an Inch, the Divisions both for some Space below,

low, and also above that, will be so very small, that the Difference of several Columns Depth of Water will not be sensible : So that an Instrument of no greater Length than this, would scarcely give an accurate Estimate of half a Mile's Depth, that is, 2640 Feet, or eighty Columns depth of Water. The lengthening therefore of this Instrument to four, five, or ten times its length, would obviate this Defect, and make the Difference of the Degrees of Descent more sensible on the Instrument. But since it is impracticable to make a metaline Tube of so great a Length, and if it were made, it would be so unweildy as to be easily broken ; it is proposed therefore to obviate these Difficulties by the following Method, *viz.*

5. Let there be a globose metaline Body be made of Copper, whose Capacity within side may be equal to nine times the Capacity of the metaline Tube : Let this globose Body be firmly screwed to the metaline Tube with a leathern Collar, well soaked in some unctuous Matter, thereby to secure that Joint in the most effectual manner.

6. There

6. There must be a Hole at the Bottom of the Copper glob Opening into a metaline Tube soldered to that Hole, which Tube must be three or four Inches length, that it may be immersed in a proper metaline Cistern, full of some tinged unctuous Matter, as Oil, or the like, which being specifically lighter than Sea Water, will swim upon it, and consequently will daub the inside of the Iron Tube, as far as the Water was impelled up it. And in order to know that Height, there must be fixed a slender Rod of Brass, Iron or Wood, in the middle of the Iron Tube from End to End, with a Knob at its upper End, both to keep it at that End in the middle of the Tube, and also to prevent its being besmeared by the sides of the Tube, when it is drawn out to see by the Mark of the unctuous Matter on the Rod to what Height the Water had been pressed up the Tube.

7. The Capacity of the Tube must be estimated by pouring Water in, when the Rod and metaline Diameter are fixed in their Places.

8. Now since the Copper Globe is supposed to contain nine times as much Air as
the

the Tube, which is the same thing as if the Tube was nine times as long, therefore the Air in the globose Vessel, will not be forced within the Capacity of the Tube, till the Vessel has descended to the Depth of nine Columns, or nine times thirty three Feet; for then the Air will be compressed within one tenth of the Space it at first took up.

9. Supposing therefore the Instrument to have descended to the Depth of ninety nine Columns of Water, or ninety nine times thirty three Feet, *viz.* 3267 Feet, then the Air will be compressed within $\frac{1}{100}$ th part of five hundred Inches (for the Capacity of the whole Vessel was supposed equal to a Tube of that length) that is, within five Inches of the Top of the Tube; and consequently the Rod will be found tinged with the coloured Oil within five Inches of its Top.

10. Suppose again the Instrument to have descended to the Depth of a hundred and ninety nine Columns of thirty three Feet each, then the Air will be compressed within the $\frac{1}{200}$ th part of the whole, that is, nearly within two and half Inches of the Top of the Tube. In this Case the Instrument will

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have

have descended 6567 Feet, that is, a Mile and a Quarter, and a hundred and thirty two Feet.

11. Suppose again the Machine to have descended to the Depth of three hundred and ninety nine Columns, then the Air will be compressed within $\frac{1}{400}$ th part of the whole, that is, nearly within one Inch and Quarter of the Top of the Tube; in this Case the Machine will have descended two Miles and half, wanting fifty three Feet; which may probably be the greatest Depth of the Sea.

12. But if there were occasion to explore greater Depths than this, it might we see be done with tolerable Accuracy, by enlarging the Capacity of the globose Vessel, which might be done without making it very cumbersome. For suppose the Diameter of the Tube were about $\frac{3}{4}$ ths of an Inch, *viz.* common Musket Barrel bore, and that it were fifty Inches long, if the globose Vessel were nineteen times as big, it would not in that case exceed the Bulk of three Gallons. But the bigger the globose Vessel, the greater care must be taken to secure well the Screw where
it

it is joined to the Tube, that no Air pass that way.

13. The bigger the globose Body is, the more weighty it ought to be, thereby the more effectually to keep it in a low depending Posture; else the Buoyancy of its contained Air might raise it as high or higher than the upper part of the Machine, whereby Water rushing in to the Top of the Tube no Observation could be made, the Rod being thereby moistened from End to End. When one Experiment has been made, the Rod and Tube must be wiped very clean, before another be repeated.

14. This Sea-Gage being thus prepared, a large Buoy must be fixed to it, which ought to be a large piece of solid Fir, or any other light solid Wood, well covered with Tar, to prevent any Waters being pressed into the Sap-Vessels; for I have found by Experience that Wood which was considerably lighter than Water, has by being greatly compressed in Water, become immediately specifically heavier than Water, for the constituent Parts of all Vegetables are specifically heavier than Water. If the Buoy be made of a Bladder or hollow Globe, with its Ori-

face inverted downwards, the Air in them will be compressed to such a Degree, at great Depths, as thereby to make the buoyant Body become specifically heavier than the Sea-Water, which would prevent its reascending to the Surface of the Sea; for which reason also the Buoy ought to be able to buoy up the Instrument when full of Water. Besides, if the Buoy when it rises again do not appear some considerable Height above the Water, it will not easily be discovered: For it is probable, that from great Depths it may rise at a considerable distance from the Ship though in a Calm. To make the Buoy therefore the more visible at a Distance, it will be proper to fix across each other, at the Top of the Buoy, broad Fans of Tin, painted either black or white, as shall be found most convenient.

15. For greater Accuracy, it will be needful first to try this Sea-Gage, at several different Depths, down to the greatest Depth that a Line can reach, thereby to discover, whether or how much the Spring of the Air is disturbed or condensed, not only by the great Pressure of the incumbent Water, but also by its Degrees of Warmth or Coldness

ness at great Depths, and in what Proportion at different known Depths, and in different lengths of time, that an Allowance may accordingly be made for it at unfathomable Depths.

16. And because it is probable that the Temper of the Air, when the Experiment is made, will be either warmer or colder, than that of the Sea at a considerable Depth, it will therefore be adviseable to let down the Instrument with a Line to a good Depth, there to continue for some time, till the Air in it may be supposed to come to the same Temper with the Sea Water: Then the Machine is to be pulled up so far above Water, as to let the Air freely pass either in or out of the Globe and Tube, according as the included Air shall either have dilated or contracted. Then instantly let the Machine loose to drop down to the Bottom of the Sea: Which it will do by means of a sinking Weight of Ballast, which must be fixed in the following manner, *viz.*

17. The sinking Weight of Ballast must be so fixed to the Machine, by means of a Catch-Hook, that as soon as the Weight touches the Ground at the Bottom of the

Sea, the Catch may then by means of a Spring, let go its hold ; whereby the Buoy will be at Liberty to carry the Machine up to the Surface of the Water.

18. The Weight of the sinking Ballast ought to be so proportioned, as to be just sufficient to sink the Machine at first, for as the Machine descends it grows continually specifically heavier, by reason of the Condensation of the Air in its Cavity, on which account its Motion will be accelerated, as well as on account of the incessant Action of the Power of Gravity upon it, so that if this gravitating Power far exceeded the contrary Renitency of the Buoy, it would strike the Bottom of the Sea with so great a Force, as might endanger the breaking of the Machine.

19. It would therefore be adviseable first to let down the Buoy with something of equal Weight with the Machine, and an Iron Rod intervening between the Machine and the Buoy, thereby to guess by the bending or not bending of the Rod, with what Degree of Force it might strike the Bottom of the Sea. And if the Force should be found to be great, I believe it might be adviseable to
fix

fix a Pole between the Machine and the Ballast, of such a Degree of Strength, that it would break before it could give Resistance enough to hurt the Machine; this would greatly break its Force against the Ground. Some of the Sand or Earth at the Bottom of the Sea should be brought up as in common Soundings, with Tallow at the Bottom of the Plummer.

20. It would be adviseable also to keep an exact Account of the time of the Machine's stay under Water, which may be done by a Watch that beats Seconds, or by a Pendulum vibrating Seconds, which must be three Feet three Inches and one fifth of an Inch long, between the middle of the Bob and the upper end of the Line.

21. Dr. *Hook* in the *Philosoph. Transact. Lowthorp's Abridgment*, Vol. 2. p. 258. found upon trial that a leaden Ball which weighed two Pounds, being fixed to a wooden Ball of the same Weight, and both let down in fourteen fathom Water, they reached the Bottom in seventeen Seconds, and the detached wooden Ball ascended to the Surface of the Water in seventeen Seconds more; so that if the Machine above described descend-

ed and ascended to greater Depths with the same Velocity, it would reach to the Depth of a Mile in seventeen Minutes, and reascend in the like time. But since the buoyant Body may return faster to the Surface of the Water than it descended, therefore Estimates from the time of the Bodies keeping under Water will be very uncertain: Yet when frequently compared with the Estimate which is made, from the height of the Water in the Gage-Tube; a Rule may perhaps be formed from thence, especially if the whole Machine be always the same, and the sinking Ballast be always of the same Weight and Size: As suppose the Ballast were put into globular earthen Vessels, made all of the same Diameter.

22. That the Sea is not many Miles deep is probable from hence, that all the great Oceans are here and there interspersed with Islands; an Argument that though as far as the sounding Line has reached, the Sea is found to be deeper and deeper, the farther from the Shore (though with some Unevenness) which would come to a great Depth indeed if it continued on so, from one boundary of the vast Oceans to the other; but
the

the interspersed Islands prove that it is not so.

23. If we suppose the Sea to deepen from the Shores in nearly the same Proportion that Land rises from the Shores, then from the following Estimate the greatest Depth of the Sea will not exceed five or six Miles. For since slow Rivers are found to have a Fall of about a Foot in a Mile, if we suppose the River *Niger* in *Africa* (which is one of the longest Rivers in the World, and runs about 2400 Miles in length) to fall at the rate of four Feet each Mile, then its whole Fall from its Rise, to its discharging itself into the Sea will be 1. 81 Miles; if it falls at the rate of six Feet each Mile, then its whole Fall will be 2. 72 Miles. If eight Feet each Mile, its Fall will be 3. 72 Miles. But if the Fall be set at ten Feet each Mile, then the Fall of the River will be 4. 54 Miles, which is a large Allowance, and may therefore well include the height of the Tops of the Mountains, from whose sides those Springs break forth: The highest Mountains being estimated to be scarce $\frac{1}{819}$ th of the Earth's semidiameter.

If

If we suppose that the whole Quantity of Earth, which is above the level of the Surface of the Seas, were equal to the whole Bulk of the Waters in the Basin of the Seas; then since the Sum of the Expanse of all the Seas is considerably more than the Sum of the Surface of all the Earth on this Globe, the general Depth of the Sea must therefore be considerably less, than the general Height of the Earth above the Surface of the Sea.

24. To this Method of taking the Depth of the Sea it may reasonably be objected, that though it has been found by Experience that in lesser Degrees the Air is compressible in Proportion to the incumbent Weights; yet it is to be feared that in greater Degrees of Compressure, this proportional Compressibility would not hold true, by reason of the many watry and other heterogeneous Mixtures in the Air, which would hinder its equable Compressibility. Yet since the irregular Degrees of its Compressibility are not yet certainly known, it might be well to try the Instrument first to the greatest Depth a Line can be let down, which I find has been to four hundred Fathom, in which case the
Air

Air would be compressed with more than seventy two Columns of Water, each equal to the Weight of the Atmosphere, so that the Air would be compressed into $\frac{1}{7}$ part of its natural Bulk, in which case the Density of that Air would be to Water as 1 to 11.64. When the Air is compressed with the Weight of ninety nine Columns of Water at the Depth of 3267 Feet, or half a Mile and 627 Feet, then its Density will be $\frac{1}{8}$ th of Water; at a hundred and ninety nine Columns Depth, or a Mile and Quarter, and a hundred and thirty two Feet, its Density will be $\frac{1}{4}$ th of Water; and at three hundred and ninety nine Columns depth, or two Miles and half wanting fifty three Feet, its Density will be $\frac{1}{2}$ of Water.

25. I have compressed Air with a Weight equal to 37.44 Atmospheres in the following manner, *viz.* I took a Glass Tube which was closed at one End, the Length of its Cavity 4.06 Inches, its Diameter 0.16. being first counterpois'd in a Scale, and then filled with Water, the Weight of the Water it contained was one Dram and six Grains. The open End of this Tube I immersed in a small Viol, at the Bottom of which was some

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Mercury

Mercury with a little Spirit of Turpentine, which was tinged with Indigo: The Viol and Tube were let down into a large Bomb full of Water, which was placed under a Cyder Press; then there was put into the Orifice of the Bomb a well turned Plug of Holly Wood, which was impelled so forcibly into the Bomb by the Screw of the Press, that Water ouzed through the Pores of the Plug, notwithstanding it had been dipped in melted Cement made of Bees Wax and Turpentine. When I took the mercurial Gage out of the Bomb, I found the tinged Turpentine had risen so high as to have compressed the included Air within the Space of 0. 12 Inch of the Top of the Tube; that is, the 38. 44th part of its Capacity; so that the included Air was compressed with 37. 44 Atmospheres, equal to the Pressure at 1235. 5 Feet or 205. 9 Fathom depth in the Sea: So that the Density of this Air was to that of Water as 1 to 22. 7.

26. Air when thus greatly compressed, has not been observed either to enter the Mercury in the Gage, nor to pass through the Pores of the Glass; nor has it been found to be fixed by any known Degrees of Compression

pressure or Cold. What Effect such an extream Pressure as two or three Miles depth of Sea Water would have on it, we can know only by Experience; which might probably be tried in the manner here proposed without much Difficulty.

27. The greatest Degree of Compressure that I have been able to give Air was in the following manner, *viz.* Having in frosty Weather placed the abovementioned Bomb with the mercurial Gage in it under the Cyder Press in the same manner as before described, I then covered the Bomb with a large Quantity of pounded Ice, which had half its Quantity of common Salt mixed with it: In a little time this great Degree of Cold burst the Bomb into three Pieces, from Top to Bottom; these Pieces opened wide, by falling asunder, but their lower Parts touched each other; an evident Proof that the Water, though compressed to so great a Degree, as to burst the Bomb, yet had very little Elasticity in it.

28. The Bomb was lined all over within side with a Shell of Ice which was about $\frac{1}{4}$ th of an Inch thick, it was full of Air Bubbles.

29. As to the Viol and mercurial Gage they were broken into many Pieces, and all the Pieces of the Tube or Gage were dawbed within side with Turpentine and Particles of Mercury, to the very Top of the Tube, which was frozen at each End into the Shell of Ice. As the Water in the middle of the Bomb was not frozen, this Experiment might easily be repeated without Danger of breaking the Gage, or Viol, *viz.* by fixing them to a small Stick, of equal Length with the Diameter of the Bomb, by which means they would be sustained within the Limits of the unfrozen Water.

30. We may from this Experiment make some Estimate of the Force which was requisite to burst the Bomb, and consequently of the Degree of Compressure which the Air in the Gage sustained, *viz.* The Diameter of the Bomb within side was $6\frac{1}{2}$ Inches, the Thickness of its Substance at its Orifice 1. 2 Inch, its Thickness at the Bottom was 1. 9 Inch. Now supposing the Substance of the Bomb to be all over of the same Thickness with its thinnest part, *viz.* 1. 2 Inch ; then the Area of the Ringlet which cuts that Substance transversely in its biggest Circle, will

will be equal to 29. 72 square Inches : That I may therefore make some Estimate of the Degree of Coherence, of the Substance of the Bomb in this Ringlet, I shall found my Calculation on Mr. *Muskhenbroek's* LXXVIIth Experiment, in his *Introductio ad Cohærentiam Corporum*, p. 505. where he found that Iron Wire, whose Diameter was $\frac{1}{10}$ th of a *Rynland* Inch, was pulled asunder with four hundred and fifty Pounds weight. The Wire being made of hammered Iron, was probably tougher than the Bomb which is made of cast Iron ; I have therefore made a large Allowance for this, in leaving a great part of the Thickness of the Bomb out of this Estimate. A *Rynland* Inch being to an *English* Inch as 0. 752 to 1, then $\frac{1}{10}$ th of the *Rynland* Inch is 0. 0752, to which 0. 133 of an *English* Inch being equal, it expresses the same Diameter of the Wire, the Area of whose transverse Section is therefore 0. 013, by which Number, if the abovementioned Area of the Ringlet, *viz.* 29. 72 be divided, the Product is 2286, which multiplied by 450, the Weight which was requisite to pull the Wire asunder, the Product is 1028700 *Amsterdam* Pounds, which being to com-

mon

mon Avoirdupoise Weight as 93 to 100, will amount to 956690 Pounds Avoirdupoise, equal to the Force requisite to burst the Bomb in halves. And the Area of the greatest Circle within the Bomb being equal to 33. 16 square Inches, and fifteen Pounds five Ounces being nearly equal to the Weight of the Atmosphere on a square Inch, these multiplied into 33. 16 square Inches give 504. 3 Pounds, the Weight of the Atmosphere on a Circle of that Area; by which dividing the above Number 956690 the Product 1837, gives the Number of Atmospheres, which pressed on the Air in the Gage; which Air must therefore be compressed into $\frac{1}{1838}$ th part of its first Space: Which is equal to the Weight of a Column of Sea Water so many times thirty three Feet high, or 60654 Feet or eleven Miles. And the length of the Cavity of the Gage being 4. 06 Inches, if the Air in it was condensed, in Proportion to the incumbent Weight, it was then compressed into 0. 0022 part of the Length of the Tube, or nearly $\frac{1}{500}$ th part of an Inch; too small a Space to be seen.

31. And since as was above observed in Experiment III. Number 13. the Air with the Pressure of one Atmosphere, rushes into a *Vacuum*, at the Rate of 1305 Feet in a Second of Time, with the Impulse of these 1837 Atmospheres, it would in *vacuo* have a Velocity of four hundred and fifty four Miles in a Second. And how much greater must the Velocity of expanding Gun Powder be, which moves with so irresistible an Impetus.

32. In order to estimate how much the Shell of the Bomb might contract by the Coldness of this freezing Mixture, I layed an Iron Back of a Fire Stove, which was of cast Iron, on a Mixture of pounded Ice and Salt, and covered it with the same, the Cold made it contract $\frac{1}{8}$ th of an Inch, i. e. $\frac{1}{120}$ th part of its Length, which was twenty five Inches. Whence by Calculation I found, the Capacity of the contracted Shell was 0.263 cubic Inch less, viz. $\frac{1}{547}$ th part less, than that of the same uncontracted Shell. And the Shell of Ice within side, at the same time dilating about $\frac{1}{10}$ th of its Bulk, the Bomb must necessarily burst.

In order to try the Incompressibleness of Water, I filled the abovementioned Bomb, before it was burst, with Pump Water, well purged of Air, which had six $+\frac{1}{2}$ Degrees of Coldness above the freezing Point: Then placing it under the Cyder Press, I pressed fast into its Orifice a Plug, which had a Hole through it from Top to Bottom of about half an Inch Diameter; into which Hole I drove with a Hammer a strong tough ashen Pin, which was covered with Cement: When the Pin which was half an Inch Diameter was so fast drove in that no Water could pass by it, it then met with so absolute a Resistance from the Water, that it seemed as if the lower end of the Pin had been drove against a Stone or solid Iron; so that on striking very hard Blows with the Hammer, the Pin was bruised to pieces between the Hammer and the resisting Water. And the Diameter of the Bomb within side, being six $+\frac{1}{2}$ Inches, the Area of its greatest Circle is 33. 06 square Inches, then the inward Surface of the Bomb will be four times that Area, *viz.* 132. 24 square Inches: This divided by 0. 196 square Inch, the Area of the Bottom of the ashen Pin, the Product

674. 7 expresses the Number of Stroaks with which all sides of the Bomb are struck, at the same Instant and with the same Force that the Hammer strikes the ashen Pin, for compressed Fluids press equally on all sides : And as these Stroaks of the Hammer would soon have distended and burst a Sphere of ductile Silver, so the Water was by this means much more forcibly compressed in this Bomb than in the Silver Sphere of the *Academy of Cimento*.

O B S E R V A T I O N XII.

I. **D**R. *Plot* in his History of *Oxfordshire* observes, that Rivers are found constantly to freeze first at their Bottoms : And the same is also observed by Watermen and Fishermen, in the *Thames* both within the reach of the Tide and also above the Tide ; who not only feel it at the Bottom with their Poles, a considerable time, *viz.* some Days before the Surface of the *Thames* is frozen over, but see it also rise up from the Bottom with such Force, as to dart up in pieces edgeways half a Foot, and sometimes a Foot above the Surface of the Water, in which Posture it continues a little time, and then turning flat on the Surface of the Water, it is carried in

great Quantities along the Stream, and is called *Ice-Meers*, which if the Frost continues, harden into one solid Surface when the River is frozen over.

2. *Jan. 30, 1730.* The Spirit in the Thermometer which hung in the open Air, being at seven a Clock in the Morning twelve Degrees below the freezing Point, and there having fallen in the Night near an Inch depth of Snow, I went to the *Thames*, and found, in a Bay at the watering Place at *Teddington*, where there was very little Stream; the Surface of the Water frozen about $\frac{2}{3}$ th of an Inch thick, under which Ice, I saw at the Bottom another Bed of Ice; then breaking away some of the upper Ice with a Paddle, I took up some of the lower Ice, which was about half Inch thick, but was not so solid as the Surface Ice, but more spongy and cavernous. This lower Ice joined to the upper Ice at the brink of the Water, and was gradually more and more distant from it, as the Bottom deepened; for it adhered close to the Bottom, where the Stones and Sand were incorporated into it, and which it brings up to the Surface along with it, when it is frozen to such a Degree,

as to be so much more specifically lighter than Water, as to be able to buoy them up with it. And when it freezes to a considerable Thickness, it will raise up with it from the Bottom, the Fishermens Oser Wheels which are sunk down with Stones or Bricks tied to them to catch Fish.

3. *Dec.* 28, 1731. At eight a Clock in the Morning, the Spirit in the Thermometer being then $12\frac{1}{2}$ Degrees below the freezing Point, I found the same Bay frozen over in like manner, both on the Surface and at the Bottom, as far as to the Edge of the quick Stream, where it was not frozen either at the Bottom or on the Top, its Motion being too brisk: And accordingly the Watermen and Fishermen observe, that it freezeth soonest at the Bottom where the Stream is most gentle; and the same may be observed as to the Surface of a Pond, which will be frozen over sooner, in that part on which a gentle North-East Wind blows, than where it is more exposed to a stronger Wind.

4. Though Snow in frosty Weather hastens the freezing of Water, yet it is observed that the *Thames* freezes first at the Bottom also when no Snow has fallen long before.

So that this cannot be owing to the sinking of the Snow to the Bottom of the River.

5. Now since this freezing at the Bottom is never observed, in Ponds and perfectly stagnating Waters, it must therefore be owing to the Motion of the Stream; for in stagnant Waters, as well as on the Earth, it is certainly colder in frosty Weather at their Surfaces than at some Depth; whereas in a flowing Stream, the upper and lower Waters being continually blended together, they are thereby brought nearly to an equal Degree of Coldness, and the upper Water being at the same time in a greater Degree of Motion than the lower, it cannot so soon freeze; for in this Bay, where the Motion of the Water was small, its Surface was frozen over, as well as the Bottom, though not with so thick an Ice: Whereas in the main River where its Motion was greater, its Surface was not frozen over, notwithstanding it was much overspread with Ice-Meers, which were continually rising from the Bottom.

6. Another Cause which may contribute to a River's freezing first at the Bottom, may be this, *viz.* That as by Experience it is found much colder to stand exposed to a
bleak

bleak Stream of sharp Air, than to stand in the same open Air under the Shelter of a Hedge or Wall; and as the Hand put into cold Water, will be sensible of a greater Degree of Cold, if the Hand be moved to and fro in the Water, than if it be held still; because in both these Cases, the stagnant Fluids which immediately touch the Body, acquire by a constant Touch a Degree of Warmth, more nearly approaching to that of the Body; whereas when there is a continued Succession of cold Fluid striking on the Body, it must needs then be affected with a greater Degree of Cold. And the Difference is the same between a River and a Pond, the Ground at the Bottom of the River being reduced to a much colder State, by the continual flow of Water over it, than the Bottom of a Pond whose Water is stagnant. And in Confirmation of this, it is observed, that it freezes first at the Bottoms, where the Stream is gentle, but not at all at the Bottom of deep Holes, whose Depth shelters them from the continued Course of the Stream: For which Reason the Fishes are observed to retreat into the deeper Places in very cold Weather.

7. And for a further Confirmation of this, there was juſt by the Bay where I made the Obſervations, a narrow Creek or Dock, as broad as two ſmall Boats, and as long as three of them; which Creek being out of any Degree of the Stream, there was no Ice at its Bottom, but the Ice on the Top was much thicker, than that on the Surface of the Water in the adjoining Bay, which were only parted by a narrow Neck of Land about two Yards wide.

O B S E R V A T I O N XIII.

I. **T**HAT the Warmth of the Earth at ſome Depth under Ground, has an Influence in promoting a Thaw, as well as the Change of the Weather from a freezing to a thawing State is manifeſt from this Obſervation, *viz.* Nov. 29, 1731, a little Snow having fallen in the Night, it was by eleven the next Morning, moſtly melted away on the Surface of the Earth, except in ſeveral Places in *Buſhy-Park*, where there were Drains dug and covered with Earth, where the Snow continued to lay, whether thoſe Drains were full of Water or dry, as alſo
where

where Elm Pipes lay under Ground; a plain Proof that these Drains intercepted the Warmth of the Earth, from ascending from greater Depths below them; for the Snow lay where the Drain had more than four Feet depth of Earth over it. It continued also to lay on Thatch, Tiles and the Tops of Walls.

F I N I S.

ERRATA.

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